BIOGRAPHIES

OF

DISTINGUISHED SCIENTIFIC MEN.

By FRANÇOIS ARAGO,

MEMBER OF THE INSTITUTE.

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TRANSLATORS' PREFACE.

THE present volume of the series of English translations of M. Arago's works consists of his own autobiography and a selection of some of his memoirs of eminent scientific men, both continental and British.

It does not distinctly appear at what period of his life Arago composed the autobiography, but it bears throughout the characteristic stamp of his ardent and energetic disposition. The reader will, perhaps, hardly suppress a smile at the indications of self-satisfaction with which several of the incidents are brought forward, while the air of romance which invests some of the adventures may possibly give rise to some suspicion of occasional embellishment; on these points, however, we leave each reader to judge for himself. In relation to the history of science, this memoir gives some interesting particulars, which disclose to us much of the interior spirit of the Academy of Sciences, not always of a kind the most creditable to some of Arago's former contemporaries.

But a far higher interest will be found to belong to those eloquent memoirs, or éloges of eminent departed men of science, who had attained the distinction of being members of the Academy.

In these the reader will find a luminous, eminently simple, and popular account of the discoveries of each of those distinguished individuals, of a kind constituting in fact a brief history of the particular branch of science to which he was devoted. And in the selection included in the present volume, which constitutes but a portion of the entire series, we have comprised the accounts of men of such varied pursuits as to convey no inadequate impression of the progress of discovery throughout a considerable range of the whole field of the physical sciences within the last half century.

The account given by the author, of the principal discoveries made by the illustrious subjects of his memoirs, is in general very luminous, but at the same time presupposes a familiarity with some parts of science which may not really be possessed by all readers. For the sake of a considerable class, then, we have taken occasion, wherever the use of new technical terms or other like circumstances seemed to require it, to introduce original notes and commentaries, sometimes of considerable extent, by the aid of which we trust the scientific principles adverted to in the text will be rendered easily intelligible to the general reader.

In some few instances also we have found ourselves called upon to adopt a more critical tone; where we were disposed to dissent from the view taken by the author on particular questions of a controversial kind, or when he is arguing in support, or in refutation, of opposing theories on some points of science not yet satisfactorily cleared up.

We could have wished that our duty as translators and editors had not extended beyond such mere occasional scientific or literary criticism. But there unfortunately seemed to be one or two points where, in pronouncing on the claims of distinguished individuals, or criticising their inventions, a doubt could not but be felt as to the perfect *fairness* of Arago's judgment, and in which we were constrained to express an unfavourable opinion on the manner in which the relative pretensions of men of the highest eminence seemed to be decided, involving what might sometimes be fairly regarded as undue prejudice, or possibly a feeling of personal or even national jealousy. Much as we should deprecate the excitement of any feeling of hostility of this kind, yet we could not, in our editorial capacity, shrink from the plain duty of endeavouring to advocate what appeared to us right and true; and we trust that whatever opinion may be entertained as to the *conclusions* to which we have come on such points, we shall not have given ground for any complaint that we have violated any due courtesy or propriety in our *mode* of expressing those conclusions, or the reasons on which they are founded.

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THE HISTORY OF MY YOUTH:

AN AUTOBIOGRAPHY OF FRANCIS ARAGO.

I HAVE not the foolish vanity to imagine that any one, even a short time hence, will have the curiosity to find out how my first education was given, and how my mind was developed; but some biographers, writing off hand and without authority, having given details on this subject utterly incorrect, and of a nature to imply negligence on the part of my parents, I consider myself bound to put them right.

I was born on the 26th of February, 1786, in the commune of Estagel, an ancient province of Roussillon (department of the Eastern Pyrenees). My father, a licentiate in law, had some little property in arable land, in vineyards, and in plantations of olive-trees, the income from which supported his numerous family.

I was thus three years old in 1789, four years old in 1790, five years in 1791, six years in 1792, and seven years old in 1793, &c.

The reader has now himself the means of judging whether, as has been said, and even stated in print, I had a hand in the excesses of our first revolution.

My parents sent me to the primary school in Estagel, where I learnt the rudiments of reading and writing. I received, besides, in my father's house, some private lessons in vocal music. I was not otherwise either more or less advanced than other children of my age. I enter into these details merely to show how much

mistaken are those who have printed that at the age of fourteen or fifteen years I had not yet learnt to read.

Estagel was a halting-place for a portion of the troops who, coming from the interior, either went on to Perpignan, or repaired direct to the army of the Pyrenees. My parents' house was therefore constantly full of officers and soldiers. This, joined to the lively excitement which the Spanish invasion had produced within me, inspired me with such decided military tastes, that my family was obliged to have me narrowly watched to prevent my joining by stealth the soldiers who left Estagel. It often happened that they caught me at a league's distance from the village, already on my way with the troops.

On one occasion these warlike tastes had nearly cost me dear. It was the night of the battle of Peires-Tortes. The Spanish troops in their retreat had partly mistaken their road. I was in the square of the village before daybreak: I saw a brigadier and five troopers come up, who, at the sight of the tree of liberty, called out, "*Somos perdidos!*" I ran immediately to the house to arm myself with a lance which had been left there by a soldier of the *levée en masse*, and placing myself in ambush at the corner of a street, I struck with a blow of this weapon the brigadier placed at the head of the party. The wound was not dangerous: a cut of the sabre, however, was descending to punish my hardihood, when some countrymen came to my aid, and, armed with forks, overturned the five cavaliers from their saddles, and made them prisoners. I was then seven years old.*

My father having gone to reside at Perpignan, as treasurer of the mint, all the family quitted Estagel to follow him there. I was then placed as an outdoor pupil at the municipal college of the town, where I occupied myself almost exclusively with my literary studies. Our classic authors had become the objects of my favourite reading. But the direction of my ideas became changed all at once by a singular circumstance which I will relate.

Walking one day on the ramparts of the town, I saw an officer of engineers who was directing the execution of the repairs. This officer, M. Cressac, was very young; I had the hardihood to approach him, and to ask him how he had succeeded in so soon wearing an epaulette. "I come from the Polytechnic School," he answered. "What school is that?" "It is a school which one enters by an examination." "Is much expected of the candidates?" "You will see it in the programme which the Government sends every year to the departmental administration; you

* With such precocious heroism it is by no means so clear that the author might not have had a hand in the revolution, from which he endeavours above to exculpate himself.

will find it moreover in the numbers of the journal of the school, which are in the library of the central school."

I ran at once to the library, and there, for the first time, I read the programme of the knowledge required in the candidates.

From this moment, I abandoned the classes of the central school, where I was taught to admire Corneille, Racine, La Fontaine, Molière, and attended only the mathematical course. This course was entrusted to a retired ecclesiastic, the Abbé Verdier, a very respectable man, but whose knowledge went no farther than the elementary course of La Caille. I saw at a glance that M. Verdier's lessons would not be sufficient to secure my admission to the Polytechnic School; I therefore decided on studying by myself the newest works, which I sent for from Paris. These were those of Legendre, Lacroix, and Garnier. In going through these works I often met with difficulties which exceeded my powers: happily, strange though it be, and perhaps without example in all the rest of France, there was a proprietor at Estagel, M. Raynal, who made the study of the higher mathematics his recreation. It was in his kitchen, whilst giving orders to numerous domestics for the labours of the next day, that M. Raynal read with advantage the "Hydraulic Architecture" of Prony, the "Mécanique Analytique," and the "Mécanique Céleste." This excellent man often gave me useful advice; but I must say that I found my real master in the cover of M. Garnier's "Treatise on Algebra." This cover consisted of a printed leaf, on the outside of which blue paper was pasted. The reading of the page not covered made me desirous to know what the blue paper hid from me. I took off this paper carefully, having first damped it, and was able to read underneath it the advice given by d'Alembert to a young man who communicated to him the difficulties which he met with in his studies:—"Go on, sir, go on, and conviction will come to you."

This gave me a gleam of light; instead of persisting in attempts to comprehend at first sight the propositions before me, I admitted their truth provisionally; I went on further, and was quite surprised, on the morrow, that I comprehended perfectly what overnight appeared to me to be encompassed with thick clouds.

I thus made myself master, in a year and a half, of all the subjects contained in the programme for admission, and I went to Montpellier to undergo the examination. I was then sixteen years of age. M. Monge, junior, the examiner, was detained at Toulouse by indisposition, and wrote to the candidates assembled at Montpellier that he would examine them in Paris. I was myself too unwell to undertake so long a journey, and I returned to Perpignan.

There I listened for a moment to the solicitations of my family, who pressed me to renounce the prospects which the Polytechnic School opened. But my taste for mathematical studies soon carried the day; I increased my library with Euler's "Introduction à l'Analyse Infinitésimale," with the "Résolution des Equations Numériques," with Lagrange's "Théorie des Fonctions Analytiques" and "Mécanique Analytique," and finally with Laplace's "Mécanique Céleste." I gave myself up with great ardour to the study of these books. From the journal of the Polytechnic School containing such investigations as those of M. Poisson on Elimination, I imagined that all the pupils were as much advanced as this geometer, and that it would be necessary to rise to this height to succeed.

From this moment, I prepared myself for the artillery service, — the aim of my ambition; and as I had heard that an officer ought to understand music, fencing, and dancing, I devoted the first hours of each day to the cultivation of these accomplishments.

The rest of the time I was seen walking in the moats of the citadel of Perpignan, seeking by more or less forced transitions to pass from one question to another, so as to be sure of being able to show the examiner how far my studies had been carried.*

At last the moment of examination arrived, and I went to Toulouse in company with a candidate who had studied at the public college. It was the first time that pupils from Perpignan had

* Méchain, member of the Academy of Sciences and of the Institute, was charged in 1792 with the prolongation of the measure of the arc of the meridian in Spain as far as Barcelona.

During his operations in the Pyrenees, in 1794, he had known my father, who was one of the administrators of the department of the Eastern Pyrenees. Later, in 1803, when the question was agitated as to the continuation of the measure of the meridian line as far as the Balearic Islands, M. Méchain went again to Perpignan, and came to pay my father a visit. As I was about setting off to undergo the examination for admission at the Polytechnic School, my father ventured to ask him whether he could not recommend me to M. Monge. "Willingly," answered he; "but, with the frankness which is my characteristic, I ought not to leave you unaware that it appears to me improbable that your son left to himself, can have rendered himself completely master of the subjects of which the programme consists. If, however, he be admitted, let him be destined for the artillery, or for the engineers; the career of the sciences, of which you have talked to me, is really too difficult to go through, and unless he had a special calling for it, your son would only find it deceptive." Anticipating a little the order of dates, let us compare this advice with what occurred: I went to Toulouse, underwent the examination, and was admitted; one year and a half afterwards I filled the situation of secretary at the Observatory, which had become vacant by the resignation of M. Méchain's son; one year and a half later, that is to say, four years after the Perpignan "horoscope," associated with M. Biot, I filled the place, in Spain, of the celebrated academician who had died there, a victim to his labours.

appeared at the competition. My intimidated comrade was completely discomfited. When I repaired after him to the board, a very singular conversation took place between M. Monge (the examiner) and me.

"If you are going to answer like your comrade, it is useless for me to question you."

"Sir, my comrade knows much more than he has shown; I hope I shall be more fortunate than he; but what you have just said to me might well intimidate me and deprive me of all my powers."

"Timidity is always the excuse of the ignorant; it is to save you from the shame of a defeat that I make you the proposal of not examining you."

"I know of no greater shame than that which you now inflict upon me. Will you be so good as to question me? it is your duty."

"You carry yourself very high, sir! We shall see presently whether this be a legitimate pride."

"Proceed, sir; I wait for you."

M. Monge then put to me a geometrical question, which I answered in such a way as to diminish his prejudices. From this he passed on to a question in algebra, to the resolution of a numerical equation. I had the work of Lagrange at my fingers' ends; I analysed all the known methods, pointing out their advantages and defects: Newton's method, the method of recurring series, the method of depression, the method of continued fractions,—all were passed in review; the answer had lasted an entire hour. Monge, brought over now to feelings of great kindness, said to me, "I could, from this moment, consider the examination at an end. I will, however, for my own pleasure, ask you two more questions. What are the relations of a curved line to the straight line which is a tangent to it?" I looked upon this question as a particular case of the theory of osculations which I had studied in Lagrange's "Fonctions Analytiques." "Finally," said the examiner to me, "how do you determine the tension of the various cords of which a funicular machine is composed?" I treated this problem according to the method expounded in the "Mécanique Analytique." It was clear that Lagrange had supplied all the resources of my examination.

I had been two hours and a quarter at the board. M. Monge, going from one extreme to the other, got up, came and embraced me, and solemnly declared that I should occupy the first place on his list. Shall I confess it? During the examination of my comrade I had heard the Toulousian candidates uttering not very favourable sarcasms on the pupils from Perpignan: and it was principally

for the sake of reparation to my native town that M. Monge's behaviour and declaration transported me with joy.

Having entered the Polytechnic School, at the end of 1803, I was placed in the excessively boisterous brigade of the Gascons and Britons. I should have much liked to study thoroughly physics and chemistry, of which I did not even know the first rudiments; but the behaviour of my companions rarely left me any time for it. As for analysis, I had already, before entering the Polytechnic School, learnt much more than was required for leaving it.

I have just related the strange words which M. Monge, junior, addressed to me at Toulouse in commencing my examination for admission. Something analogous occurred at the opening of my examination in mathematics for passing from one division of the school to another. The examiner, this time, was the illustrious geometer Legendre, of whom, a few years after, I had the honour of becoming the colleague and the friend.

I entered his study at the moment when M. T——, who was to undergo his examination before me, having fainted away, was being carried out in the arms of two servants. I thought that this circumstance would have moved and softened M. Legendre; but it had no such effect. "What is your name," he said to me sharply. "Arago," I answered. "You are not French then?" "If I was not French I should not be before you; for I have never heard of any one being admitted into the school unless his nationality had been proved." "I maintain that he is not French whose name is Arago." "I maintain, on my side, that I am French, and a very good Frenchman too, however strange my name may appear to you." "Very well; we will not discuss the point farther; go to the board."

I had scarcely taken up the chalk, when M. Legendre, returning to the first subject of his preoccupations, said to me: "You were born in one of the departments recently united to France?" "No, sir; I was born in the department of the Eastern Pyrenees, at the foot of the Pyrenees." "Oh! why did you not tell me that at once? all is now explained. You are of Spanish origin, are you not?" "Possibly; but in my humble family there are no authentic documents preserved which could enable me to trace back the civil position of my ancestors: each one there is the child of his own deeds. I declare to you again that I am French, and that ought to be sufficient for you."

The vivacity of this last answer had not disposed M. Legendre in my favour. I saw this very soon; for, having put a question to me which required the use of double integrals, he stopped me, saying: "The method which you are following was not given to

you by the professor. Whence did you get it?" "From one of your papers." "Why did you choose it? was it to bribe me?" "No; nothing was farther from my thoughts. I only adopted it because it appeared to me preferable." "If you are unable to explain to me the reasons for your preference, I declare to you that you shall receive a bad mark, at least as to character."

I then entered upon the details which established, as I thought, that the method of double integrals was in all points more clear and more rational than that which Lacroix had expounded to us in the amphitheatre. From this moment Legendre appeared to me to be satisfied, and to relent.

Afterwards, he asked me to determine the centre of gravity of a spherical sector. "The question is easy," I said to him. "Very well; since you find it easy, I will complicate it: instead of supposing the density constant, I will suppose that it varies from the centre to the surface according to a determined function." I got through this calculation very happily: and from this moment I had entirely gained the favour of the examiner. Indeed, on my retiring, he addressed to me these words, which, coming from him, appeared to my comrades as a very favourable augury for my chance of promotion: "I see that you have employed your time well: go on in the same way the second year, and we shall part very good friends."

In the mode of examination adopted at the Polytechnic School in 1804, which is always cited as being better than the present organisation, room was allowed for the exercise of some unjustifiable caprices. Would it be believed, for example, that the old M. Barruel examined two pupils at a time in physics, and gave them, it is said, the same mark, which was the mean between the actual merits of the two? For my part, I was associated with a comrade full of intelligence, but who had not studied this branch of the course. We agreed that he should leave the answering to me, and we found the arrangement advantageous to both.

As I have been led to speak of the school as it was in 1804, I will say that its faults were less those of organisation than those of personal management; for many of the professors were much below their office, a fact which gave rise to somewhat ridiculous scenes. The pupils, for instance, having observed the insufficiency of M. Hassenfratz, made a demonstration of the dimensions of the rainbow, full of errors of calculation, but in which the one compensated the other so that the final result was true. The professor, who had only this result whereby to judge of the goodness of the answer, when he saw it appear on the board, did not hesitate to call out, "Good, good, perfectly good!" which excited shouts of laughter on all the benches of the amphitheatre.

When a professor has lost consideration, without which it is impossible for him to do well, they allow themselves to insult him to an incredible extent. Of this I will cite a single specimen.

A pupil, M. Leboullenger, met one evening in company this same M. Hassenfratz, and had a discussion with him. When he re-entered the school in the morning, he mentioned this circumstance to us. "Be on your guard," said one of our comrades to him; "you will be interrogated this evening. Play with caution, for the professor has certainly prepared some great difficulties so as to cause laughter at your expense."

Our anticipations were not mistaken. Scarcely had the pupils arrived in the amphitheatre, when M. Hassenfratz called to M. Leboullenger, who came to the board.

"M. Leboullenger," said the professor to him, "you have seen the moon?" "No, sir." "How, sir! you say that you have never seen the moon?" "I can only repeat my answer — no, sir." Beside himself, and seeing his prey escape him, by means of this unexpected answer, M. Hassenfratz addressed himself to the inspector charged with the observance of order that day, and said to him, "Sir, there is M. Leboullenger who pretends never to have seen the moon." "What would you wish me to do?" stoically replied M. Le Brun. Repulsed on this side, the professor turned once more towards M. Leboullenger, who remained calm and earnest in the midst of the unspeakable amusement of the whole amphitheatre, and cried out with undisguised anger, "You persist in maintaining that you have never seen the moon?" "Sir," returned the pupil, "I should deceive you if I told you that I had not heard it spoken of, but I have never seen it." "Sir, return to your place."

After this scene, M. Hassenfratz was but a professor in name; his teaching could no longer be of any use.

At the commencement of the second year, I was appointed "*chef de brigade.*" Hatchette had been professor of hydrography at Collioure; his friends from Roussillon recommended me to him. He received me with great kindness, and even gave me a room in his lodgings. It was there that I had the pleasure of making Poisson's acquaintance, who lived next to us. Every evening the great geometer entered my room, and we passed entire hours in conversing on politics and mathematics, which is certainly not quite the same thing.

In the course of 1804, the school was a prey to political passions, and that through the fault of the government.

They wished forthwith to oblige the pupils to sign an address of congratulation on the discovery of the conspiracy in which

Moreau was implicated. They refused to do so on the ground that it was not for them to pronounce on a cause which had been in the hands of justice. It must, however, be remarked, that Moreau had not yet dishonoured himself by taking service in the Russian army, which had come to attack the French under the walls of Dresden.

The pupils were invited to make a manifestation in favour of the institution of the Legion of Honour. This again they refused. They knew well that the cross, given without inquiry and without control, would be, in most cases, the recompense of charlatanism, and not of true merit.

The transformation of the Consular into the Imperial Government gave rise to very animated discussions in the interior of the school.

Many pupils refused to add their felicitations to the mean adulations of the constituted bodies.

General Lacuée, who was appointed governor of the school, reported this opposition to the Emperor.

"M. Lacuée," cried Napoleon, in the midst of a group of courtiers, who applauded with speech and gesture, "you cannot retain at the school those pupils who have shown such ardent Republicanism: you will send them away." Then, collecting himself, he added, "I will first know their names and their stages of promotion." Seeing the list the next day, he did not proceed further than the first name, which was the first in the artillery. "I will not drive away the first men in advancement," said he. "Ah! if they had been at the bottom of the list! M. Lacuée, leave them alone."

Nothing was more curious than the *séance* to which General Lacuée came to receive the oath of obedience from the pupils. In the vast amphitheatre which contained them, one could not discern a trace of the gravity which such a ceremony should inspire. The greater part, instead of answering, at the call of their names, "I swear it," cried out, "Present."

All at once, the monotony of this scene was interrupted by a pupil, son of the Conventionalist Brissot, who called out in a stentorian voice, "I will not take the oath of obedience to the Emperor." Lacuée, pale and with little presence of mind, ordered a detachment of armed pupils placed behind him to go and arrest the recusant. The detachment, of which I was at the head, refused to obey. Brissot, addressing himself to the General, with the greatest calmness said to him, "Point out the place to which you wish me to go; do not force the pupils to dishonour themselves by laying hands on a comrade who has no desire to resist."

The next morning Brissot was expelled.

About this time, M. Méchain, who had been sent to Spain to prolong the meridional line as far as Formentera, died at Castellon de la Plana. His son, Secretary at the Observatory, immediately gave in his resignation. Poisson offered me the situation. I declined his first proposal. I did not wish to renounce the military career,—the object of all my predilections, and in which, moreover, I was assured of the protection of Marshal Lannes,—a friend of my father's. Nevertheless I accepted, on trial, the position offered me in the Observatory, after a visit which I made to M. de Laplace in company with M. Poisson, under the express condition that I could re-enter the Artillery if that should suit me. It was from this cause that my name remained inscribed on the list of the pupils of the school. I was only detached to the Observatory on a special service.

I entered this establishment, then, on the nomination of Poisson, my friend, and through the intervention of Laplace. The latter loaded me with civilities. I was happy and proud when I dined in the Rue de Tournon with the great geometer. My mind and my heart were much disposed to admire all, to respect all, that was connected with him who had discovered the cause of the secular equation of the moon, had found in the movement of this planet the means of calculating the ellipticity of the earth, had traced to the laws of attraction the long inequalities of Jupiter and of Saturn, &c. &c. But what was my disenchantment, when one day I heard Madame de Laplace, approaching her husband, say to him, "Will you entrust to me the key of the sugar?"

Some days afterwards, a second incident affected me still more vividly. M. de Laplace's son was preparing for the examinations of the Polytechnic School. He came sometimes to see me at the Observatory. In one of his visits I explained to him the method of continued fractions, by help of which Lagrange obtains the roots of numerical equations. The young man spoke of it to his father with admiration. I shall never forget the rage which followed the words of Emile de Laplace, and the severity of the reproaches which were addressed to me, for having patronised a mode of proceeding which may be very long in theory, but which evidently can in no way be found fault with on the score of its elegance and precision. Never had a jealous prejudice shown itself more openly, or under a more bitter form. "Ah!" said I to myself, "how true was the inspiration of the ancients when they attributed weaknesses to him who nevertheless made Olympus tremble by a frown!"

Here I should mention, in order of time, a circumstance which might have produced the most fatal consequences for me. The fact was this:—

I have described above, the scene which caused the expulsion of Brissot's son from the Polytechnic School. I had entirely lost sight of him for several months, when he came to pay me a visit at the Observatory, and placed me in the most delicate, the most terrible, position that an honest man ever found himself in.

"I have not seen you," he said to me, "because since leaving the school I have practised daily firing with a pistol; I have now acquired a skill beyond the common, and I am about to employ it in ridding France of the tyrant who has confiscated all her liberties. My measures are taken: I have hired a small room on the Carrousel, close to the place by which Napoleon, on coming out from the court, will pass to review the cavalry; from the humble window of my apartment will the ball be fired which will go through his head."

I leave it to be imagined with what despair I received this confidence. I made every imaginable effort to deter Brissot from his sinister project: I remarked how all those who had rushed on enterprises of this nature had been branded in history by the odious title of assassin. Nothing succeeded in shaking his fatal resolution; I only obtained from him a promise on his honour that the execution of it should be postponed for a time, and I put myself in quest of means for rendering it abortive.

The idea of announcing Brissot's project to the authorities did not even enter my thoughts. It seemed a fatality which came to smite me, and of which I must undergo the consequences, however serious they might be.

I counted much on the solicitations of Brissot's mother, already so cruelly tried during the revolution. I went to her home, in the Rue de Condé, and implored her earnestly to cooperate with me in preventing her son from carrying out his sanguinary resolution. "Ah, sir," replied this lady, who was naturally a model of gentleness, "if Silvain" (this was the name of her son) "believes that he is accomplishing a patriotic duty, I have neither the intention nor the desire to turn him from his project."

It was from myself that I must henceforth draw all my resources. I had remarked that Brissot was addicted to the composition of romances and pieces of poetry. I encouraged this passion, and every Sunday, above all, when I knew that there would be a review, I went to fetch him, and drew him into the country, in the environs of Paris. I listened then complacently to the reading of those chapters of his romance which he had composed during the week.

The first excursions frightened me a little, for armed with his pistols, Brissot seized every occasion of showing his great skill; and I reflected that this circumstance would lead to my being con-

sidered as his accomplice, if he ever carried out his project. At last, his pretensions to literary fame, which I flattered to the utmost, the hopes (though I had none myself) which I led him to conceive of the success of an attachment of which he had confided the secret to me, made him receive with attention the reflections which I constantly made to him on his enterprise. He determined on making a journey beyond the seas, and thus relieved me from the most serious anxiety which I have experienced in all my life.

Brissot died after having covered the walls of Paris with printed handbills in favour of the Bourbon restoration.

I had scarcely entered the Observatory, when I became the fellow-labourer of Biot in researches on the refraction of gases, already commenced by Borda.

While engaged in this work the celebrated academician and I often conversed on the interest there would be in resuming in Spain the measurement interrupted by the death of Méchain. We submitted our project to Laplace, who received it with ardour, procured the necessary funds, and the Government confided to us two this important mission.

M. Biot, I, and the Spanish commissary Rodriguez departed from Paris in the commencement of 1806. We visited, on our way, the stations indicated by Méchain: we made some important modifications in the projected triangulation, and at once commenced operations.

An inaccurate direction given to the reflectors established at Iviza, on the mountain Campvey, rendered the observations made on the continent extremely difficult. The light of the signal of Campvey was very rarely seen, and I was, during six months, in the *Desierto de las Palmas,* without being able to see it, whilst at a later period the light established at the Desierto, but well directed, was seen every evening from Campvey. It will easily be imagined what must be the *ennui* experienced by a young and active astronomer, confined to an elevated peak, having for his walk only a space of twenty square metres, and for diversion only the conversation of two Carthusians, whose convent was situated at the foot of the mountain, and who came in secret, infringing the rule of their order.

At the time when I write these lines, old and infirm, my legs scarcely able to sustain me, my thoughts revert involuntarily to that epoch of my life when, young and vigorous, I bore the greatest fatigues, and walked day and night, in the mountainous countries which separate the kingdoms of Valencia and Catalonia from the kingdom of Aragon, in order to reestablish our geodesic signals which the storms had overset.

I was at Valencia towards the middle of October, 1806. One morning early the French consul entered my room quite alarmed: "Here is sad news," said M. Lanusse to me; "make preparations for your departure; the whole town is in agitation; a declaration of war against France has just been published; it appears that we have experienced a great disaster in Prussia. The Queen, we are assured, has put herself at the head of the cavalry and of the royal guard; a part of the French army has been cut to pieces; the rest is completely routed. Our lives would not be in safety if we remained here; the French ambassador at Madrid will inform me as soon as an American vessel now at anchor in the 'Grao' of Valencia can take us on board, and I will let you know as soon as the moment is come." This moment never came; for a few days afterwards the false news, which one must suppose had dictated the proclamation of the Prince of the Peace, was replaced by the bulletin of the battle of Jéna. People who at first played the braggart and threatened to root us out suddenly became disgracefully cast down: we could walk in the town, holding up our heads, without fear henceforth of being insulted.

This proclamation, in which they spoke of the critical circumstances in which the Spanish nation was placed; of the difficulties which encompassed this people; of the safety of their native country; of laurels, and of the god of victory; of enemies with whom they ought to fight; — did not contain the name of France. They availed themselves of this omission (will it be believed?) to maintain that it was directed against Portugal.

Napoleon pretended to believe in this absurd interpretation; but from this moment it became evident that Spain would sooner or later be obliged to render a strict account of the warlike intentions which she had suddenly evinced in 1806; this, without justifying the events of Bayonne, explains them in a very natural way.

I was expecting M. Biot at Valencia, he having undertaken to bring some new instruments with which we were to measure the latitude of Formentera. I shall take advantage of these short intervals of repose to insert here some details of manners, which may, perhaps, be read with interest.

I will recount, in the first instance, an adventure which nearly cost me my life under somewhat singular circumstances.

One day, as a recreation, I thought I could go, with a fellow-countryman, to the fair at Murviedro, the ancient Saguntum, which they told me was very curious. I met in the town the daughter of a Frenchman resident at Valencia, Madlle. B——. All the hotels were crowded; Madlle. B —— invited us to take

some refreshments at her grandmother's; we accepted; but on leaving the house she informed us that our visit had not been to the taste of her betrothed, and that we must be prepared for some sort of attack on his part: we went directly to an armourer's, bought some pistols, and commenced our return to Valencia.

On our way I said to the calezero (driver), a man whom I had employed for a long time, and who was much devoted to me: —

"Isidro, I have some reason to believe that we shall be stopped; I warn you of it, so that you may not be surprised at the shots which will be fired from the caleza (vehicle)."

Isidro, seated on the shaft, according to the custom of the country, answered: —

"Your pistols are completely useless, gentlemen; leave me to act; one cry will be enough; my mule will rid us of two, three, or even four men."

Scarcely one minute had elapsed after the calezero had uttered these words, when two men presented themselves before the mule and seized her by the nostrils. At the same instant a formidable cry, which will never be effaced from my remembrance, — the cry of *Capitana!* — was uttered by Isidro. The mule reared up almost vertically, raising up one of the men, came down again, and set off at a rapid gallop. The jolt which the carriage made led us to understand too well what had just occurred. A long silence succeeded this incident; it was only interrupted by these words of the calezero, "Do you not think, gentleman, that my mule is worth more than any pistols?"

The next day the captain-general, Don Domingo Izquierdo, related to me that a man had been found crushed on the road to Murviedro. I gave him an account of the prowess of Isidro's mule, and no more was said.

One anecdote, taken from among a thousand, will show what an adventurous life was led by the delegate of the *Bureau of Longitude.*

During my stay on a mountain near Cullera, to the north of the mouth of the river Xucar, and to the south of the Albuféra, I once conceived the project of establishing a station on the high mountains which are in front of it. I went to see them. The alcaid of one of the neighbouring villages warned me of the danger to which I was about to expose myself. "These mountains," said he to me, "form the resort of a band of highway robbers." I asked for the national guard, as I had the power to do so. My escort was supposed by the robbers to be an expedition directed against them, and they dispersed themselves at once over the rich plain which is watered by the Xucar. On my return I found them engaged in combat with the authorities of

Cullera. Wounds had been given on both sides, and, if I recollect right, one alguazil was left dead on the plain.

The next morning I regained my station. The following night was a horrible one; the rain fell in a deluge. Towards night, there was knocking at my cabin door. To the question, "Who is there?" the answer was, "A custom-house guard, who asks of you a shelter for some hours." My servant having opened the door to him, I saw a magnificent man enter, armed to the teeth. He laid himself down on the earth, and went to sleep. In the morning, as I was chatting with him at the door of my cabin, his eyes flashed on seeing two persons on the slope of the mountain, the alcaid of Cullera and his principal alguazil, who were coming to pay me a visit. "Sir," cried he, "nothing less than the gratitude which I owe to you, on account of the service which you have rendered to me this night, could prevent my seizing this occasion for ridding myself, by one shot of this carabine, of my most cruel enemy. Adieu, sir!" And he departed, springing from rock to rock as light as a gazelle.

On reaching the cabin, the alcaid and his alguazil recognised in the fugitive the chief of all the brigands in the country.

Some days afterwards, the weather having again become very bad, I received a second visit from the pretended custom-house guard, who went soundly to sleep in my cabin. I saw that my servant, an old soldier, who had heard the recital of the deeds and behaviour of this man, was preparing to kill him. I jumped down from my camp bed, and, seizing my servant by the throat,—"Are you mad?" said I to him; "are we to discharge the duties of police in this country? Do you not see, moreover, that this would expose us to the resentment of all those who obey the orders of this redoubted chief? And we should thus render it impossible for us to terminate our operations."

Next morning, when the sun rose, I had a conversation with my guest, which I will try to reproduce faithfully.

"Your situation is perfectly known to me; I know that you are not a custom-house guard; I have learnt from certain information that you are the chief of the robbers of the country. Tell me whether I have anything to fear from your confederates?"

"The idea of robbing you did occur to us; but we concluded that all your funds would be in the neighbouring towns; that you would carry no money to the summit of mountains, where you would not know what to do with it, and that our expedition against you could have no fruitful result. Moreover, we cannot pretend to be as strong as the King of Spain. The King's troops leave us quietly enough to exercise our industry; but on the day that we molested an envoy from the Emperor of the French, they

would direct against us several regiments, and we should soon have to succumb. Allow me to add, that the gratitude which I owe to you is your surest guarantee."

"Very well, I will trust in your words; I shall regulate my conduct by your answer. Tell me if I can travel at night? It is fatiguing to me to move from one station to another in the day under the burning influence of the sun."

"You can do so, sir; I have already given my orders to this purpose; they will not be infringed."

Some days afterwards, I left for Denia; it was midnight, when some horsemen rode up to me, and addressed these words to me:—

"Stop there, señor; times are hard: those who have something must aid those who have nothing. Give us the keys of your trunks; we will only take your superfluities."

I had already obeyed their orders, when it came into my head to call out—"But I have been told, that I could travel without risk."

"What is your name, sir?"

"Don Francisco Arago."

"*Hombre! vaya usted con Dios* (God be with you)."

And our cavaliers, spurring away from us, rapidly lost themselves in a field of "algarrobos."

When *my friend* the robber of Cullera assured me that I had nothing to fear from his subordinates, he informed me at the same time that his authority did not extend north of Valencia. The banditti of the northern part of the kingdom obeyed other chiefs; one of whom, after having been taken, was condemned and hung, and his body divided into four quarters, which were fastened to posts, on four royal roads, but not without their having previously been boiled in oil, to make sure of their longer preservation.

This barbarous custom produced no effect; for scarcely was one chief destroyed before another presented himself to replace him.

Of all these brigands those had the worst reputation who carried on their depredations in the environs of Oropeza. The proprietors of the three mules, on which M. Rodriguez, I, and my servant were riding one evening in this neighbourhood, were recounting to us the "grand deeds" of these robbers, which, even in full daylight, would have made the hair of one's head stand on end, when, by the faint light of the moon, we perceived a man hiding himself behind a tree; we were six, and yet this sentry on horseback had the audacity to demand our purses or our lives: my servant at once answered him—"You must then believe us to be very cowardly; take yourself off, or I will bring you down by one shot of my carabine." "I will be off," returned the worth-

less fellow, "but you will soon hear news of me." Still full of fright at the remembrance of the stories which they had just been relating, the three "arieros" besought us to quit the high road and cast ourselves into a wood which was on our left. We yielded to their proposal; but we lost our way. "Dismount," said they, "the mules have been obeying the bridle and you have directed them wrongly. Let us retrace our way as far as the high road, and leave the mules to themselves, they will well know how to find their right way again." Scarcely had we effected this manœuvre, which succeeded marvellously well, when we heard a lively discussion taking place at a short distance from us. Some were saying: "We must follow the high road, and we shall meet with them." Others maintained that they must get into the wood on the left. The barking of the dogs, by which these individuals were accompanied, added to the tumult. During this time we pursued our way silently, more dead than alive. It was two o'clock in the morning. All at once we saw a faint light in a solitary house; it was like a light-house for the mariner in the midst of the tempest, and the only means of safety which remained to us. Arrived at the door of the farm, we knocked and asked for hospitality. The inmates, very little reassured, feared that we were thieves, and did not hurry themselves to open to us.

Impatient at the delay, I cried out, as I had received authority to do so, "In the name of the King, open to us!" They obeyed an order thus given; we entered pell-mell, and in the greatest haste, men and mules, into the kitchen, which was on the ground-floor; and we hurried to extinguish the lights, in order not to awaken the suspicions of the bandits who were seeking for us. Indeed, we heard them, passing and repassing near the house, vociferating with the whole force of their lungs against their unlucky fate. We did not quit this solitary house until broad day, and we continued our route for Tortosa, not without having given a suitable recompense to our hosts. I wished to know by what providential circumstance they happened to have a lamp burning at that unseasonable hour. "We had killed a pig," they told me, "in the course of the day, and we were busy preparing the black puddings." Had the pig lived one day more, or had there been no black puddings, I should certainly have been no longer in this world, and I should not have the opportunity to relate the story of the robbers of Oropeza.

Never could I better appreciate the intelligent measure by which the constituent assembly abolished the ancient division of France into provinces, and substituted its division into departments, than in traversing for my triangulation the Spanish border kingdoms of Catalonia, Valencia, and Aragon. The inhabitants of these

three provinces detested each other cordially, and nothing less than the bond of a common hatred was necessary to make them act simultaneously against France. Such was their animosity in 1807 that I could scarcely make use at the same time of Catalonians, Aragons, and Valencians, when I moved with my instruments from one station to another. The Valencians, in particular, were treated by the Catalonians as a light, trifling, inconsistent people. They were in the habit of saying to me, "*En el reino de Valencia la carne es verdura, la verdura agua, los hombres mugeres, las mugeres nada;* which may be translated thus: — "In the kingdom of Valencia meat is a vegetable, vegetables are water, men are women, and women nothing."

On the other hand, the Valencians, speaking of the Aragons, used to call them "*schuros.*"

Having asked of a herdsman of this province who had brought some goats near to one of my stations, what was the origin of this denomination, at which his compatriots showed themselves so offended:

"I do not know," said he, smiling cunningly at me, "whether I dare answer you." "Go on, go on," I said to him, "I can hear anything without being angry." "Well, the word *schuros* means that, to our great shame, we have sometimes been governed by French kings. The sovereign, before assuming power, was bound to promise under oath to respect our freedom and to articulate in a loud voice the solemn words *lo Juro!* As he did not know how to pronounce the J he said *schuro.* Are you satisfied, senor?" I answered him, "Yes, yes. I see that vanity and pride are not dead in this country."

Since I have just spoken of a shepherd, I will say that in Spain, the class of individuals of both sexes destined to look after herds, appeared to me always less further removed than in France, from the pictures which the ancient poets have left us of the shepherds and shepherdesses in their pastoral poetry. The songs by which they endeavour to while away the tedium of their monotonous life, are more remarkable in their form and substance than in the other European nations to which I have had access. I never recollect without surprise, that being on a mountain situated at the junction-point of the kingdoms of Valencia, Aragon, and Catalonia, I was all at once overtaken by a violent storm, which forced me to take refuge in my tent, and to remain there squatting on the ground. When the storm was over and I came out from my retreat, I heard, to my great astonishment, on an isolated peak which looked down upon my station, a shepherdess who was singing a song of which I only recollect these eight lines, which will give an idea of the rest: —

* * * * *

A los que amor no saben
Ofreces las dulzuras
Y a mi las amarguras
Que s'e lo que es amar.

Las gracias al me certé
Eran cuadro de flores
Te cantaban amores
Por hacerte callar.

Oh! how much sap there is in this Spanish nation! What a pity that they will not make it yield fruit!

In 1807, the tribunal of the Inquisition existed still at Valencia, and at times performed its functions. The reverend fathers, it is true, did not burn people, but they pronounced sentences in which the ridiculous contended with the odious. During my residence in this town, the holy office had to busy itself about a pretended sorceress; it doomed her to go through all quarters of the town astride on an ass, her face turned towards the tail, and naked down to the waist. Merely to observe the commonest rules of decency, the poor woman had been plastered with a sticky substance, partly honey, they told me, to which adhered an enormous quantity of little feathers, so that, to say the truth, the victim resembled a fowl with a human head. The procession, whether attended by a crowd I leave it to be imagined, stationed itself for some time in the cathedral square, where I lived. I was told that the sorceress was struck on the back a certain number of blows with a shovel; but I do not venture to affirm this, for I was absent at the moment when this hideous procession passed before my windows.

We thus see, however, what sort of spectacles were given to the people in the commencement of the nineteenth century, in one of the principal towns of Spain, the seat of a celebrated university, and the native country of numerous citizens distinguished by their knowledge, their courage, and their virtues. Let not the friends of humanity and of civilisation disunite; let them form, on the contrary, an indissoluble union, for superstition is always on the watch, and waits for the moment again to seize its prey.

I have mentioned in the course of my narrative that two Carthusians often left their convent in the *Desierto de las Palmas*, and came, though prohibited, to see me at my station, situated about two hundred metres higher. A few particulars will give an idea of what certain monks were, in the Peninsula, in 1807.

One of them, Father Trivulce, was old; the other was very young. The former, of French origin, had played a part at Marseilles, in the counter-revolutionary events of which this town

was the theatre, at the commencement of our first revolution His part had been a very active one; one might see the proo of this in the scars of sabre cuts which furrowed his breast. I was he who was the first to come. When he saw his young comrade march up, he hid himself; but as soon as the latte had fully entered into conversation with me, Father Trivulc showed himself all at once. His appearance had the effect o Medusa's head. "Reassure yourself," said he to his young com peer; "only let us not denounce each other, for our prior is not a man to pardon us for having come here and infringed our vow o silence, and we should both receive a punishment, the recollectio of which would long remain." The treaty was at once concluded and from that day forward the two Carthusians came very ofte to converse with me.

The youngest of our two visitors was an Aragonian, his family had made him a monk against his will. He related to me one day before M. Biot (then returned from Tarragon, where he had taken refuge to get cured of his fever), some particulars which, according to him, proved that in Spain there was no longer more than the ghost of religion. These details were mostly borrowed from the secrets of confession. M. Biot manifested sharply the displeasure which this conversation caused him; there were even in his language some words which led the monk to suppose that M. Biot took him for a kind of spy. As soon as this suspicion had entered his mind, he quitted us without saying a word, and the next morning I saw him come up early, armed with a light gun. The French monk had preceded him, and had whispered in my ear the danger that threatened my companion. "Join with me," he said, "to turn the young Aragonian monk from his murderous project." I need scarcely say that I employed myself with ardour in this negotiation, in which I had the happiness to succeed. There were here, as must be seen, the materials for a chief of *guerilleros.* I should be much astonished if my young monk did not play his part in the war of independence.

The anecdote which I am about to relate will amply prove that religion was, with the Carthusian monks of the *Desierto de las Palmas,* not the consequence of elevated sentiments, but a mere compound of superstitious practices.

The scene with the gun, always present to my mind, seemed to make it clear to me that the Aragon monk, if actuated by his passions, would be capable of the most criminal actions. Hence, I had a very disagreeable impression when one Sunday, having come down to hear mass, I met this monk, who, without saying a word, conducted me by a series of dark corridors into a chapel

where the daylight penetrated only by a very small window. There I found Father Trivulce, who prepared himself to say mass for me alone. The young monk assisted. All at once, an instant before the consecration, Father Trivulce, turning towards me, said these exact words: —"We have permission to say mass with white wine; we therefore make use of that which we gather from our own vines: this wine is very good. Ask the prior to let you taste it, when on leaving this you go to breakfast with him. For the rest, you can assure yourself this instant of the truth of what I say to you." And he presented me the goblet to drink from. I resisted strongly, not only because I considered it indecent to give this invitation in the middle of the mass, but because, besides, I must own I conceived the thought for a moment that the monks wished, by poisoning me, to revenge themselves on me for M. Biot having insulted them. I found that I was mistaken, that my suspicions had no foundation; for Father Trivulce went on with the interrupted mass, drank, and drank largely, of the white wine contained in one of the goblets. But when I had got out of the hands of the two monks, and was able to breathe the pure air of the country, I experienced a lively satisfaction.

The right of asylum accorded to some churches was one of the most obnoxious privileges among those of which the revolution of 1789 rid France. In 1807, this right still existed in Spain, and belonged, I believe, to all the cathedrals. I learnt, during my stay at Barcelona, that there was, in a little cloister contiguous to the largest church of the town, a brigand,— a man guilty of several assassinations, who lived quietly there, guaranteed against all pursuit by the sanctity of the place. I wished to assure myself with my own eyes of the reality of the fact, and I went with my friend Rodriguez into the little cloister in question. The assassin was then eating a meal which a woman had just brought him. He easily guessed the object of our visit, and made immediately such demonstrations as convinced us that, if the asylum was safe for the robber, it would not be so long for us. We retired at once, deploring that in a country calling itself civilised, there should still exist such crying, such monstrous abuses.

In order to succeed in our geodesic operations, to obtain the co-operation of the inhabitants of the villages near our stations, it was desirable for us to be recommended to the priests. We went, therefore,— M. Lanusse, the French Vice-Consul, M. Biot, and I,— to pay a visit to the Archbishop of Valencia, to solicit his protection. This archbishop, a man of very tall figure, was then chief of the Franciscans; his costume more than negligent,

his grey robe, covered with tobacco, contrasted with the magnificence of the archiepiscopal palace. He received us with kindness, and promised us all the recommendations we desired; but, at the moment of taking leave of him, the whole affair seemed to be spoiled. M. Lanusse and M. Biot went out of the reception room without kissing the hand of his grace, although he had presented it to each of them very graciously. The archbishop indemnified himself on my poor person. A movement, which was very near breaking my teeth, a gesture which I might justly call a blow of the fist, proved to me that the chief of the Franciscans, notwithstanding his vow of humility, had taken offence at the want of ceremony in my fellow visitors. I was going to complain of the abrupt way in which he had treated me, but I had the necessities of our trigonometrical operations before my eyes, and I was silent.

Besides this, at the instant when the closed fist of the archbishop was applied to my lips, I was still thinking of the beautiful optical experiments which it would have been possible to make with the magnificent stone which ornamented his pastoral ring. This idea, I must frankly declare, had preoccupied me during the whole of the visit.

M. Biot having at last come to seek me again at Valencia, where I expected, as I have before said, some new instruments, we went on to Formentera, the southern extremity of our arc, of which place we determined the latitude. M. Biot quitted me afterwards to return to Paris, whilst I made the geodesical junction of the island of Majorca to Iviza, and to Formentera, obtaining thus, by means of one single triangle, the measure of an arc of parallel of one degree and a half.

I then went to Majorca, to measure there the latitude and the azimuth.

At this epoch, the political fermentation, engendered by the entrance of the French into Spain, began to invade the whole Peninsula and the islands dependent on it. This ferment had as yet in Majorca only reached to the ministers, the partisans, and the relations of the Prince of Peace. Each evening, I saw, drawn in triumph in the square of Palma, the capital of the island of Majorca, on carriages, the effigies in flames, sometimes of the minister Soller, another time those of the bishop, and even those of private individuals supposed to be attached to the fortunes of the favourite Godoï. I was far from suspecting then that my turn would soon arrive.

My station at Majorca, the *Clop de Galazo*, a very high mountain, was situated exactly over the port where *Don Jayme el Conquistator* disembarked when he went to deliver the Balearic

Islands from the Moors. The report spread itself through the population that I had established myself there in order to favour the arrival of the French army, and that every evening I made signals to it. But these reports had nothing menacing until the moment of the arrival at Palma, the 27th of May, 1808, of an ordnance officer from Napoleon. This officer was M. Berthémie; he carried to the Spanish squadron, at Mahon, the order to go in all haste to Toulon. A general rising, which placed the life of this officer in danger, followed the news of his mission. The Captain-General Vivés only saved his life by shutting him up in the strong castle of Belver. They then bethought themselves of the Frenchman established on the *Clop de Galazo*, and formed a popular expedition to go and seize him.

M. Damian, the owner of a small kind of vessel called a Mistic, which the Spanish Government had placed at my disposal, was beforehand with them, and brought me a costume by means of which I disguised myself. In directing myself towards Palma, in company with this brave seaman, we met with the rioters, who were going in search of me. They did not recognize me, for I spoke Majorcan perfectly. I strongly encouraged the men of this detachment to continue their route, and I pursued my way towards Palma. At night I went on board the Mistic, commanded by Don Manuel de Vacaro, whom the Spanish Government had placed under my orders. I asked this officer if he would conduct me to Barcelona, occupied by the French, promising him that if they made any attempt to keep him there, I would at once return and surrender myself a prisoner.

Don Manuel, who up to this time had shown extreme obsequiousness towards me, had now no words but those of rudeness and distrust. There occurred on the pier where the Mistic was moored a riotous movement, which Vacaro assured me was directed against me. "Do not be uneasy," said he to me; "if they should penetrate into the vessel you can hide yourself in this trunk." I made the attempt; but the chest which he showed me was so small that my legs were entirely outside, and the cover could not be shut down. I understood perfectly what that meant, and I asked M. Vacaro to let me also be shut up in the castle of Belver. The order for incarceration having arrived from the captain general, I got into the boat, where the sailors of the Mistic received me with emotion.

At the moment of their crossing the harbour the populace perceived me, commenced a pursuit, and it was not without much difficulty that I reached Belver safe and sound. I had only, indeed, received on my way one slight wound from a dagger in the thigh. Prisoners have often been seen to run with all speed *from* their dungeon; I am the first, perhaps, to whom it has

happened to do the reverse. This took place on the 1st or 2nd of June, 1808.

The governor of Belver was a very extraordinary personage. If he is still alive he may demand of me a certificate as to his priority to the modern hydropathists: the grenadier-captain maintained that pure water, suitably administered, was a means of treatment for all illnesses, even for amputations. By listening very patiently to his theories, and never interrupting him, I won his good opinion. It was at his request, and from interest in our safety, that a Swiss garrison replaced the Spanish troop which until then had been employed as the guard of Belver. It was also through him that I one day learnt that a monk had proposed to the soldiers who went to bring my food from the town, to put some poison into one of the dishes.

All my old Majorcan friends had abandoned me at the moment of my detention. I had had a very sharp correspondence with Don Manuel de Vacaro in order to obtain the restitution of the passport of safety which the English Admiralty had granted to us. M. Rodriguez alone ventured to visit me in full daylight, and bring me every consolation in his power.

The excellent M. Rodriguez, to while away the monotony of my incarceration, remitted to me from time to time the journals which were then published at different parts of the Peninsula. He often sent them to me without reading them. Once I saw in these journals the recital of the horrible massacres of which the town of Valencia,—I make a mistake, the *square of the Bull-fights*—had been the theatre, and in which nearly the whole of the French established in this town (more than 350) had disappeared under the pike of the bull-fighter. Another journal contained an article bearing this title: "Relacion de la ahorcadura del señor Arago e del señor Berthemie,"—literally, "Account of the execution of M. Arago and M. Berthémie." This account spoke of the two executed men in very different terms. M. Berthemie was a Huguenot; he had been deaf to all exhortations; he had spit in the face of the ecclesiastic who was present, and even on the image of Christ. As for me, I had conducted myself with much decency, and had allowed myself to be hung without giving rise to any scandal. The writer also expressed his regret that a young astronomer had been so weak as to associate himself with treason, coming under the disguise of science to assist the entrance of the French army into a friendly kingdom.

After reading this article I immediately made my decision: "Since they talk of my death," said I to my friend Rodriguez, "the event will not be long in coming. I should prefer being

drowned to being hung. I will make my escape from this fortress; it is for you to furnish me with the means."

Rodriguez, knowing better than any one how well founded my apprehensions were, set himself at once to the work.

He went to the captain-general, and made him feel what would be the danger of his position if I should disappear in a popular riot, or even if he were forced to give me up. His observations were so much the better comprehended, as no one could then predict what might be the issue of the Spanish revolution. "I will undertake," said the captain-general Vivés to my colleague Rodriguez, "to give an order to the commander of the fortress, that when the right moment arrives, he shall allow M. Arago, and even the two or three other Frenchmen who are with him in the castle of Belver, to pass out. They will then have no need of the means of escape which they have procured; but I will take no part in the preparations which will become necessary to enable the fugitives to leave the island; I leave all that to your responsibility."

Rodriguez immediately conferred secretly with the brave commander Damian. It was agreed between them that Damian should take the command of a half-decked boat, which the wind had driven ashore; that he should equip it as if for a fishing expedition; that he should carry us to Algiers; after which his re-entrance at Palmas, with or without fish, would inspire no suspicion.

All was executed according to agreement, notwithstanding the inquisitorial surveillance which Don Manuel de Vacaro exercised over the commander of his "Mistic."

On the 28th July, 1808, we silently descended the hill on which Belver is built, at the same moment that the family of the minister Soller entered the fortress to escape the fury of the populace. Arrived at the shore, we found there Damian, his boat, and three sailors. We embarked at once, and set sail. Damian had taken the precaution of bringing with us in this frail vessel the instruments of value which he had carried off from my station at the Clop de Galazo. The sea was unfavourable; Damian thought it prudent to stop at the little island of Cabrera, destined to become a short time afterwards so sadly celebrated by the sufferings which the soldiers of the army of Dupont experienced after the shameful capitulation of Baylen. There a singular incident was very near compromising all. Cabrera, tolerably near to the southern extremity of Majorca, is often visited by fishermen coming from that part of the island. M. Berthémie feared, justly enough, that the rumour of our escape having spread about, they might dispatch some boats to seize us. He looked upon our going into harbour as inopportune; I maintained that we must yield to the prudence

of the commander. During this discussion, the three seamen whom Damian had engaged saw that M. Berthémie, whom I had endeavoured to pass off as my servant, maintained his opinion against me on a footing of equality. They then addressed themselves in these terms to the commander: —

"We only consented to take part in this expedition upon condition that the Emperor's aide-de-camp, shut up at Belver, should not be of the number of those persons whom we should help off. We only wished to aid the flight of the astronomer. Since it seems to be otherwise, you must leave this officer here, unless you would prefer to throw him into the sea."

Damian at once informed me of the imperative wishes of his boat's crew. M. Berthémie agreed with me to suffer some abuse such as could only be tolerated by a servant threatened by his master; all the suspicions disappeared.

Damian, who feared also for himself the arrival of Majorcan fishermen, hastened to set sail on the 29th of July, 1808, the first moment that was favourable, and we arrived at Algiers on the 3rd of August.

Our looks were anxiously directed towards the port, to guess what reception might await us. We were reassured by the sight of the tri-coloured flag, which was flying on two or three buildings. But we were mistaken; these buildings were Dutch. Immediately upon our entrance, a Spaniard, whom, from his tone of authority, we took for a high functionary of the Regency, came up to Damian, and asked him: "What do you bring?" "I bring," answered the commander, "four Frenchmen." "You will at once take them back again. I prohibit you from disembarking." As we did not seem inclined to obey his order, our Spaniard, who was the constructing engineer of the ships of the Dey, armed himself with a pole, and commenced battering us with blows. But immediately a Genoese seaman, mounted on a neighbouring vessel, armed himself with an oar, and struck our assailant both with edge and point. During this animated combat we managed to land without any opposition. We had conceived a singular idea of the manner in which the police act on the coast of Africa.

We pursued our way to the French Consul's, M. Dubois Thainville. He was at his country house. Escorted by the janissary of the consulate, we went off towards this country house, one of the ancient residences of the Dey, situated not far from the gate of Bab-azoum. The consul and his family received us with great amity, and offered us hospitality.

Suddenly transported to a new continent, I looked forward anxiously to the rising of the sun to enjoy all that Africa might offer of interest to a European, when all at once I believed my-

self to be engaged in a serious adventure. By the faint light of the dawn, I saw an animal moving at the foot of my bed. I gave a kick with my foot: all movement ceased. After some time, I felt the same movement made under my legs. A sharp jerk made this cease quickly. I then heard the fits of laughter of the janissary, who lay on a couch in the same room as I did; and I soon saw that he had simply placed on my bed a large hedgehog to amuse himself by my uneasiness.

The consul occupied himself the next day in procuring a passage for us on board a vessel of the Regency which was going to Marseilles. M. Ferrier, the Chancellor of the French Consulate, was at the same time Consul for Austria. He procured for us two false passports, which transformed us—M. Berthémie and me—into two strolling merchants, the one from *Schwekat*, in Hungary, the other from *Leoben*.

The moment of departure had arrived; the 13th of August, 1808, we were on board, but our ship's company was not complete. The captain, whose title was Raï Braham Ouled Mustapha Goja, having perceived that the Dey was on his terrace, and fearing punishment if he should delay to set sail, completed his crew at the expense of the idlers who were looking on from the pier, and of whom the greater part were not sailors. These poor people begged as a favour for permission to go and inform their families of this precipitate departure, and to get some clothes. The captain remained deaf to their remonstrances. We weighed anchor.

The vessel belonged to the Emir of Seca, Director of the Mint. The real commander was a Greek captain, named Spiro Calligero. The cargo consisted of a great number of *groups*. Amongst the passengers there were five members of the family which the Bakri had succeeded as kings of the Jews; two ostrich feather merchants, Moroccans; Captain Krog, from Berghen in Norway, who had sold his ship at Alicant; two lions sent by the Dey to the Emperor Napoleon, and a great number of monkeys. Our voyage was prosperous. Off Sardinia we met with an American ship coming out from Cagliari. A cannon-shot (we were armed with forty pieces of small power) warned the captain to come to be recognised. He brought on board a certain number of counterparts of passports, one of which agreed perfectly with that which we carried. The captain being thus all right, was not a little astonished when I ordered him, in the name of Captain Braham, to furnish us with tea, coffee, and sugar. The American captain protested; he called us brigands, pirates, robbers. Captain Braham admitted without difficulty all these qualifications, and persisted none the less in the exaction of sugar, coffee, and tea.

The American, then driven to the last stage of exasperation,

addressed himself to me, who acted as interpreter, and cried out, "Oh! rogue of a renegade! if ever I meet you on holy ground I will break your head." "Can you then suppose," I answered him, "that I am here for my pleasure, and that, notwithstanding your menace, I would not rather go with you, if I could?" These words calmed him; he brought the sugar, the coffee, and the tea claimed by the Moorish chief and we again set sail, though without having exchanged the usual farewell.

We had already entered the Gulf of Lyons, and were approaching Marseilles, when on the 16th August, 1808, we met with a Spanish corsair from Palamos, armed at the prow with two 24-pounders. We made full sail; we hoped to escape it: but a cannon shot, a ball from which went through our sails, taught us that she was a much better sailer than we were.

We obeyed an injunction thus expressed, and awaited the great boat from the corsair. The captain declared that he made us prisoners, although Spain was at peace with Barbary, under the pretext that we were violating the blockade which had been lately raised on all the coasts of France: he added, that he intended to take us to Rosas, and that there the authorities would decide on our fate.

I was in the cabin of the vessel; I had the curiosity to look furtively at the crew of the boat, and there I perceived, with a dissatisfaction which may easily be imagined, one of the sailors of the "Mistic," commanded by Don Manuel de Vacaro, of the name of Pablo Blanco, of Palamos, who had often acted as my servant during my geodesic operations. My false passport would become from this moment useless, if Pablo should recognise me: I went to bed at once, covered my head with the counterpane, and lay as still as a statue.

During the two days which elapsed between our capture and our entrance into the roads of Rosas, Pablo, whose curiosity often brought him into the room, used to exclaim, "There is one passenger whom I have not yet managed to get a sight of."

When we arrived at Rosas it was decided that we should be placed in quarantine in a dismantled windmill, situated on the road leading to Figueras. I was careful to disembark in a boat to which Pablo did not belong. The corsair departed for a new cruise, and I was for a moment freed from the harassing thoughts which my old servant had caused me.

Our ship was richly laden; the Spanish authorities were immediately desirous to declare it a lawful prize. They pretended to believe that I was the proprietor of it, and wished, in order to hasten things, to interrogate me, even without awaiting the completion of the quarantine. They stretched two cords between

the mill and the shore, and a judge placed himself in front of me. As the interrogatories were made from a good distance, the numerous audience which encircled us took a direct part in the questions and answers. I will endeavour to re-produce this dialogue with all possible fidelity: —

"Who are you?"

"A poor roving merchant."

"Whence do you come?"

"From a country where you certainly never were."

"In a word, what country is it?"

I was afraid to answer, for the passports, steeped in vinegar, were in the hands of the judge-instructor, and I had forgotten whether I was from Schwekat or from Leoben. Finally I answered at all hazards: —

"I come from Schwekat."

And this information happily was found to agree with that of the passport.

"You are as much from Schwekat as I am," answered the judge. "You are Spanish, and, moreover, a Spaniard from the kingdom of Valencia, as I perceive by your accent."

"Would you punish me, sir, because nature has endowed me with the gift of languages? I learn with facility the dialects of those countries through which I pass in the exercise of my trade; I have learnt, for example, the dialect of Iviza."

"Very well, you shall be taken at your word. I see here a soldier from Iviza; you shall hold a conversation with him."

"I consent; I will even sing the goat song."

Each of the verses of this song (if verses they be) terminates by an imitation of the bleating of the goat.

I commenced at once, with an audacity at which I really feel astonished, to chant this air, which is sung by all the shepherds of the island.

Ah graciada señora
Una canzo bouil canta
Bè, bè, bè, bè.

No sera gaira pulida
Nosé si vos agradara
Bè, bè, bè, bè.

At once my Ivizacan, upon whom this air had the effect of the *ranz des vaches* on the Swiss, declared, all in tears, that I was a native of Iviza.

I then said to the judge that if he would put me in communication with a person knowing the French language, he would arrive at just as embarrassing a result. An *émigré* officer of the

Bourbon regiment offered at once to make the experiment, and, after some phrases interchanged between us, affirmed without hesitation that I was French.

The judge, rendered impatient, exclaimed, "Let us put an end to these trials which decide nothing. I summon you, sir, to tell me who you are. I promise that your life will be safe if you answer me with sincerity."

"My greatest wish would be to give an answer to your satisfaction. I will, then, try to do so; but I warn you that I am not going to tell you the truth. I am son of the innkeeper at Mataró." "I know that innkeeper; you are not his son." "You are right. I announced to you that I should vary my answers until one of them should suit you. I retract then, and tell you that I am a *titiretero* (player of marionettes), and that I practised at Lerida."

A loud shout of laughter from the multitude encircling us greeted this answer, and put an end to the questions.

"I swear by the d——l," exclaimed the judge, that I will discover sooner or later who you are!"

And he retired.

The Arabs, the Moroccans, the Jews, who witnessed this interrogatory, understood nothing of it; they had only seen that I had not allowed myself to be intimidated. At the close of the interview they came to kiss my hand, and gave me, from this moment, their entire confidence.

I became their secretary for all the individual or collective remonstrances which they thought they had a right to address to the Spanish Government; and this right was incontestable. Every day I was occupied in drawing up petitions, especially in the name of the two ostrich feather merchants, one of whom called himself a tolerably near relation of the Emperor of Morocco. Astonished at the rapidity with which I filled a page of my writing, they imagined, doubtless, that I should write as fast in Arabic characters, when it should be requisite to transcribe passages from the Koran; and that this would form both for me and for them the source of a brilliant fortune, and they besought me, in the most earnest way, to become a Mahomedan.

Very little reassured by the last words of the judge, I sought means of safety from another quarter.

I was the possessor of a safe-conduct from the English Admiralty; I therefore wrote a confidential letter to the captain of an English vessel, the "Eagle," I think, which had cast anchor some days before in the roads at Rosas. I explained to him my position. "You can," I said to him, "claim me, because I have an English passport. If this proceeding should cost you too much,

have the goodness at least to take my manuscripts and to send them to the Royal Society in London."

One of the soldiers who guarded us, and in whom I had fortunately inspired some interest, undertook to deliver my letter. The English captain came to see me: his name was, if my memory is right, George Eyre. We had a private conversation on the shore. George Eyre thought, perhaps, that the manuscripts of my observations were contained in a register bound in morocco, and with gilt edges to the leaves. When he saw that these manuscripts were composed of single leaves, covered with figures which I had hidden under my shirt, disdain succeeded to interest, and he quitted me hastily. Having returned on board, he wrote me a letter which I could find if needful, in which he said to me,—"I cannot mix myself up in your affairs: address yourself to the Spanish Government; I am persuaded that it will do justice to your remonstrance, and will not molest you." As I had not the same persuasion as Captain George Eyre, I chose to take no notice of his advice.

I ought to mention that some time after having related these particulars in England, at Sir Joseph Banks's, the conduct of George Eyre was severely blamed; but when a man breakfasts and dines to the sound of harmonious music, can he accord his interest to a poor devil sleeping on straw and nibbled by vermin, even though he have manuscripts under his shirt? I may add that I (unfortunately for me) had to do with a captain of an unusual character. For, some days later, a new vessel, the "Colossus," having arrived in the roads, the Norwegian, Captain Krog, although he had not, like me, an Admiralty passport, made an application to the commander of this new ship; he was immediately claimed, and relieved from captivity.

The report that I was a Spanish deserter, and proprietor of the vessel, acquiring more and more credit, and this position being the most dangerous of all, I resolved to get out of it. I begged the commandant of the place, M. Alloy, to come to receive my declaration, and I announced to him that I was French. To prove to him the truth of my words, I invited him to send for Pablo Blanco, the sailor in the service of the corsair who took us, and who had returned from his cruise a short time before. This was done as I wished. In disembarking, Pablo Blanco, who had not been warned, exclaimed with surprise: "What! you, Don Francisco, mixed up with all these miscreants!" The sailor gave the Governor circumstantial evidence as to the mission which I fulfilled with two Spanish commissaries. My nationality thus became proved.

That same day Alloy was replaced in the command of the

fortress by the Irish Colonel of the Ultonian regiment; the corsair left for a fresh cruise, taking away Pablo Blanco; and I became once more the roving merchant from Schwekat.

From the windmill, where we underwent our quarantine, I could see the tricoloured flag flying on the fortress of Figueras. The reconnoitring parties of the cavalry came sometimes within five or six hundred metres; it would not then have been difficult for me to escape. However, as the regulations against those who violate the sanitary laws are very rigorous in Spain, as they pronounce the penalty of death against him who infringes them, I only determined to make my escape on the eve of our admission to pratique.

The night being come I crept on all-fours along the briars, and I should soon have got beyond the line of sentinels who guarded us. A noisy uproar which I heard among the Moors made me determine to re-enter, and I found these poor people in an unspeakable state of uneasiness, thinking themselves lost if I left; I therefore remained.

The next day a strong picquet of troops presented itself before the mill. The manœuvres made by it inspired all of us with anxiety, but especially Captain Krog.* "What will they do with us?" he exclaimed. "Alas! you will see only too soon," replied the Spanish officer. This answer made every one believe that they were going to shoot us. What might have strengthened me in this idea was the obstinacy with which Captain Krog and two other individuals of small size hid themselves behind me. A handling of arms made us think that we had but a few seconds to live.

In analysing the feelings which I experienced on this solemn occasion, I have come to the conclusion that the man who is led to death is not as unhappy as the public imagines him to be. Fifty ideas presented themselves nearly simultaneously to my mind, and I did not rack my brain for any of them; I only recollect the two following, which have remained engraved on my memory. On turning my head to the right, I saw the national flag flying on the bastions of Figueras, and I said to myself, "If I were to move a few hundred metres, I should be surrounded by comrades, by friends, by fellow citizens, who would receive me affectionately. Here, without their being able to impute any crime to me, I am going to suffer death at twenty-two years of age." But what agitated me more deeply was this: looking towards the Pyrenees, I could distinctly see their peaks, and I

* This appears to be an oversight, as in a preceding page M. Arago described the fortunate release of Captain Krog from this captivity.

reflected that my mother, on the other side of the chain, might at this awful moment be looking peaceably at them.

The Spanish authorities, finding that to redeem my life I would not declare myself the owner of the vessel, had us conducted without farther molestation to the fortress of Rosas. Having to file through nearly all the inhabitants of the town, I had wished at first, through a false feeling of shame, to leave in the mill the remains of our week's meals. But M. Berthémie, more prudent than I, carried over his shoulder a great quantity of pieces of black bread, tied up with packthread. I imitated him. I furnished myself famously from our old stock, set it on my shoulder, and it was with this accoutrement that I made my entrance into the famous fortress.

They placed us in a casemate, where we had barely the space necessary for lying down. In the windmill, they used to bring us, from time to time, some provisions, which came from our boat. Here, the Spanish government purveyed our food. We received every day some bread and a ration of rice; but as we had no means of dressing food, we were in reality reduced to dry bread.

Dry bread was very unsubstantial food for one who could see from his casemate, at the door of his prison, a sutler selling grapes at two farthings a pound, and cooking, under the shelter of half a cask, bacon and herrings; but we had no money to bring us into connection with this merchant. I then decided, though with very great regret, to sell a watch which my father had given me. I was only offered about a quarter of its value; but I might well accept it, since there were no competitors for it.

As possessors of sixty francs, M. Berthémie and I could now appease the hunger from which we had long suffered; but we did not like this return of fortune to be profitable to ourselves alone, and we made some presents, which were very well received by our companions in captivity. Though this sale of my watch brought some comfort to us, it was doomed at a later period to plunge a family into sorrow.

The town of Rosas fell into the power of the French after a courageous resistance. The prisoners of the garrison were sent to France, and naturally passed through Perpignan. My father went in quest of news wherever Spaniards were to be found. He entered a café at the moment when a prisoner officer drew from his fob the watch which I had sold at Rosas. My good father saw in this act the proof of my death, and fell into a swoon. The officer had got the watch from a third party, and could give no account of the fate of the person to whom it had originally belonged.

The casemate having become necessary to the defenders of the

fortress, we were taken to a little chapel, where they deposited for twenty-four hours those who had died in the hospital. There we were guarded by peasants who had come across the mountain, from various villages, and particularly from Cadaquès. These peasants, eager to recount all that they had seen of interest during their one day's campaign, questioned me as to the deeds and behaviour of all my companions in misfortune. I satisfied their curiosity amply, being the only one of the set who could speak Spanish.

To enlist their good will, I also questioned them at length upon the subject of their village, on the work that they did there, on smuggling, their principal sources of employment, &c. &c. They answered my questions with the loquacity common to country rustics. The next day our guards were replaced by some others who were inhabitants of the same village. "In my business of a roving merchant," I said to these last, "I have been at Cadaquès;" and then I began to talk to them of what I had learnt the night before, of such an individual, who gave himself up to smuggling with more success than others, of his beautiful residence, of the property which he possessed near the village,—in short, of a number of particulars which it seemed impossible for any but an inhabitant of Cadaquès to know. My jest produced an unexpected effect. Such circumstantial details, our guards said to themselves, cannot be known by a roving merchant; this personage, whom we have found here in such singular society, is certainly a native of Cadaquès; and the son of the apothecary must be about his age. He had gone to try his fortune in America: it is evidently he who fears to make himself known, having been found with all his riches in a vessel on its way to France. The report spread, became more consistent, and reached the ears of a sister of the apothecary established at Rosas. She runs to me, believes she recognises me, and falls on my neck. I protest against the identity. "Well played!" said she to me; "the case is serious, as you have been found in a vessel coming to France; persist in your denial; circumstances may perhaps take a more favourable turn, and I shall profit by them to insure your deliverance. In the mean time, my dear nephew, I will let you want for nothing." And truly every morning M. Berthémie and I received a comfortable repast.

The church having become necessary to the garrison to serve as a magazine, we were moved on the 25th of September, 1808, to a Trinity fort, called the *Bouton de Rosas*, a citadel situated on a little mountain at the entrance of the roads, and we were deposited deep under ground, where the light of day did not penetrate on any side. We did not long remain in this infected place; not

because they had pity upon us, but because it offered shelter for a part of the garrison attacked by the French. They made us descend by night to the edge of the sea, and then transported us on the 17th of October to the port of Palamos. We were shut up in a hulk: we enjoyed, however, a certain degree of liberty; — they allowed us to go on land, and to parade our miseries and our rags in the town. It was there that I made the acquaintance of the dowager Duchess of Orleans, mother of Louis Philippe. She had left the town of Figueras, where she resided, because, she told me, thirty-two bombs sent from the fortress had fallen in her house. She was then intending to take refuge in Algiers, and she asked me to bring the captain of the vessel to her, of whom, perhaps, she would have to implore protection. I related to my "*raïs*" the misfortunes of the Princess; he was moved by them, and I conducted him to her. On entering, he took off his slippers from respect, as if he had entered within a mosque, and holding them in his hand, he went to kiss the front of the dress of Madame d'Orleans. The Princess was alarmed at the sight of this manly figure, wearing the longest beard I ever saw; she quickly recovered herself, and the interview proceeded with a mixture of French politeness and Oriental courtesy.

The sixty francs from Rosas were expended. Madame D'Orleans would have liked much to assist us, but she was herself without money. All that she could gratify us with was a piece of sugar-bread. The evening of our visit I was richer than the Princess. To avoid the fury of the people the Spanish Government sent those French who had escaped the first massacres back to France in slight boats. One of the *cartels* came and cast anchor by the side of our hulk. One of the unhappy emigrants offered me a pinch of snuff. On opening the snuff-box I found there "*una onza de oro*" (an ounce of gold), the sole remains of his fortune. I returned the snuff-box to him, with warm thanks, after having shut up in it a paper containing these words; — "My fellow-countryman who carries this note has rendered me a great service; — treat him as one of your children." My petition was naturally favourably received; it was by this bit of paper, the size of the *onza de oro*, that my family learnt that I was still in existence, and it enabled my mother — a model of piety — to cease saying mssses for the repose of my soul.

Five days afterwards, one of my hardy compatriots arrived at Palamos, after having traversed the line of posts both French and Spanish, carrying to a merchant who had friends at Perpignan the proposal to furnish me with all I was in need of. The Spaniard showed a great inclination to agree to the proposal; but I did not

profit by his good will, because of the occurrence of events which I shall relate presently.

The Observatory at Paris is very near the barrier. In my youth, curious to study the manners of the people, I used to walk in sight of the public-houses which the desire of escaping payment of the duty has multiplied outside the walls of the capital: on these excursions I was often humiliated to see men disputing for a piece of bread, just as animals might have done. My feelings on this subject have very much altered since I have been personally exposed to the tortures of hunger. I have discovered, in fact, that a man, whatever may have been his origin, his education, and his habits, is governed, under certain circumstances, much more by his stomach than by his intelligence and his heart. Here is the fact which suggested these reflections to me.

To celebrate the unhoped-for arrival of *una onza de oro*, M. Berthémie and I had procured an immense dish of potatoes. The ordnance officer of the Emperor was already devouring it with his eyes, when a Moroccan, who was making his ablutions near us with one of his companions, accidentally filled it with dirt. M. Berthémie could not control his anger: he darted upon the clumsy Mussulman, and inflicted upon him a rough punishment.

I remained a passive spectator of the combat, until the second Moroccan came to the aid of his compatriot. The party no longer being equal, I also took part in the conflict by seizing the new assailant by the beard. The combat ceased at once, because the Moroccan would not raise his hand against a man who could write a petition so rapidly. This conflict, like the struggles of which I had often been a witness outside the barriers of Paris, had originated in a dish of potatoes.

The Spaniards always cherished the idea that the ship and her cargo might be confiscated; a commission came from Girone to question us. It was composed of two civil judges and one inquisitor. I acted as interpreter. When M. Berthémie's turn came, I went to fetch him, and said to him, "Pretend that you can only talk Styrian, and be at ease; I will not compromise you in translating your answers."

It was done as we had agreed; unfortunately the language spoken by M. Berthémie had but little variety, and the *sacrement der Teufel*, which he had learnt in Germany, when he was aide-de-camp to Hautpoul, predominated too much in his discourse. Be that as it may, the judges observed that there was too great a conformity between his answers and those which I had made myself, to render it necessary to continue an interrogatory, which I may say, by the way, disturbed me much. The wish to terminate it was still more decided on the part of the judges, when

it came to the turn of a sailor named Mehemet. Instead of making him swear on the Koran to tell the truth, the judge was determined to make him place his thumb on the fore-finger so as represent the cross. I warned him that great offence would thus be given; and, accordingly, when Mehemet became aware of the meaning of this sign, he began to spit upon it with inconceivable violence. The meeting ended at once.

The next day things had wholly changed their appearance; one of the judges from Girone came to declare to us that we were free to depart, and to go with our ship wherever we chose. What was the cause of this sudden change? It was this.

During our quarantine in the windmill at Rosas, I had written, in the name of Captain Braham, a letter to the Dey of Algiers. I gave him an account of the illegal arrest of his vessel, and of the death of one of the lions which the Dey had sent to the Emperor. This last circumstance transported the African monarch with rage. He sent immediately for the Spanish Consul, M. Onis, claimed pecuniary damages for his dear lion, and threatened war if his ship was not released directly. Spain had then to do with too many difficulties to undertake wantonly any new ones, and the order to release the vessel so anxiously coveted arrived at Girone, and from thence at Palamos.

This solution, to which our Consul at Algiers, M. Dubois Thainville, had not remained inattentive, reached us at the moment when we least expected it. We at once made preparations for our departure, and on the 28th of November, 1808, we set sail, steering for Marseilles: but, as the Mussulmen on board the vessel declared, it was written above that we should not enter that town. We could already perceive the white buildings which crown the neighbouring hills of Marseilles, when a gust of the "mistral," of great violence, sent us from the north towards the south.

I do not know what route we followed, for I was lying in my cabin, overcome with sea-sickness; I may therefore, though an astronomer, avow without shame, that at the moment when our unqualified pilots supposed themselves to be off the Baléares, we landed, on the 5th of December, at Bougie.

There, they pretended that during the three months of winter, all communication with Algiers, by means of the little boats named *sandalis*, would be impossible, and I resigned myself to the painful prospect of so long a stay in a place at that time almost a desert. One evening I was making these sad reflections while pacing the deck of the vessel, when a shot from a gun on the coast came and struck the side planks close to which I was

passing. This suggested to me the thought of going to Algiers by land.

I went next day, accompanied by M. Berthémie and Captain Spiro Calligero, to the Caïd of the town: "I wish," said I to him, "to go to Algiers by land." The man, quite frightened, exclaimed, "I cannot allow you to do so; you would certainly be killed on the road; your Consul would make a complaint to the Dey, and I should have my head cut off."

"Fear not on that ground. I will give you an acquittance."

It was immediately drawn up in these terms: "We, the undersigned, certify that the Caïd of Bougie wished to dissuade us from going to Algiers by land; that he has assured us that we shall be massacred on the road; that notwithstanding his representations, reiterated twenty times, we have persisted in our project. We beg the Algerine authorities, particularly our Consul, not to make him responsible for this event if it should occur. We once more repeat, that the voyage has been undertaken against his will.

"*Signed:* ARAGO and BERTHÉMIE."

Having given this declaration to the Caïd, we considered ourselves quit of this functionary; but he came up to me, undid, without saying a word, the knot of my cravat, took it off, and put it into his pocket. All this was done so quickly that I had not time, I will add that I had not even the wish, to reclaim it.

At the conclusion of this audience, which had terminated in so singular a manner, we made a bargain with a Mahomedan priest, who promised to conduct us to Algiers for the sum of twenty "piastres fortes" and a red mantle. The day was occupied in disguising ourselves well or ill, and we set out the next morning, accompanied by several Moorish sailors belonging to the crew of the ship, after having shown the Mahomedan priest that we had nothing with us worth a sou, so that if we were killed on the road he would inevitably lose all reward.

I went, at the last moment, to make my bow to the only lion that was still alive, and with whom I had lived in very good harmony; I wished also to say good-bye to the monkeys, who during nearly five months had been equally my companions in misfortune.* These monkeys during our frightful misery had rendered us a service which I scarcely dare mention, and which

* On my return to Paris I hastened to the Jardin des Plantes to pay a visit to the lion, but he received me with a very unamiable gnashing of the teeth. Think then of the marvellous history of the Florentine lion, the subject of so many engravings, which is offered on the stall of every printseller to the eyes of the moved and astonished passers by.

will scarcely be guessed by the inhabitants of our cities, who look upon these animals as objects of diversion; they freed us from the vermin which infested us, and showed particularly a remarkable cleverness in seeking out the hideous insects which lodged themselves in our hair.

Poor animals! they seemed to me very unfortunate in being shut up in the narrow enclosure of the vessel, when, on the neighbouring coast, other monkeys, as if to bully them, came on to the branches of the trees, giving innumerable proofs of their agility.

At the commencement of the day, we saw on the road two Kabyls, similar to the soldiers of Jugurtha, whose harsh appearance powerfully allayed our fancy for wandering. In the evening we witnessed a fearful tumult, which appeared to be directed against us. We learnt afterwards that the Mahomedan priest had been the object of it; that it originated with some Kabyls whom he had disarmed on one of their journeys to Bougie. This incident, which appeared likely to be repeated, inspired us for a moment with the thought of returning; but the sailors were resolute, and we continued our hazardous enterprise.

In proportion as we advanced, our troops became increased by a certain number of Kabyls, who wished to go to Algiers to work there in the quality of seamen, and who dared not undertake alone this dangerous journey.

The third day we encamped in the open air, at the entrance of a forest. The Arabs lighted a very large fire in the form of a circle, and placed themselves in the middle. Towards eleven o'clock, I was awakened by the noise which the mules made, all trying to break their fastenings. I asked what was the cause of this disturbance. They answered me that a "*sebâá*" had come roaming in the neighbourhood. I was not aware then that a "*sebâá*" was a lion, and I went to sleep again. The next day, in traversing the forest, the arrangement of the caravan was changed. It was grouped in the smallest space possible; one Kabyl was at the head, his gun ready for service; another was in the rear, in the same position. I inquired of the owner of the mule the cause of these unusual precautions. He answered me, that they were dreading an attack from a "*sebâá*," and that if this should occur, one of us would be carried off without having time to put himself on the defensive. "I would rather be a spectator," I said to him, "than an actor in the scene you describe; consequently, I will give you two piastres more if you will keep your mule always in the centre of the moving group." My proposal was accepted. It was then for the first time that I saw that my Arab carried a yatagan under his tunic, which he used for prick-

ing on the mule the whole time that we were in the thicket. Superfluous cautions! The "*sebââ*" did not show himself.

Each village being a little republic, whose territory we could not cross without obtaining permission and a passport from the Mahomedan priest *président*, the priest who conducted our caravan used to leave us in the fields, and went sometimes a good way off to a village to solicit the permission without which it would have been dangerous to continue our route. He remained entire hours without returning to us, and we then had occasion to reflect sadly on the imprudence of our enterprise. We generally slept amongst habitations. Once, we found the streets of a village barricaded, because they were fearing an attack from a neighbouring village. The foremost man of our caravan removed the obstacles; but a woman came out of her house like a fury, and belaboured us with blows from a pole. We remarked that she was fair, of brilliant whiteness, and very pretty.

Another time we lay down in a lurking-place dignified by the beautiful name of caravanseray. In the morning, when the sun rose, cries of "*Roumi! Roumi!*" warned us that we had been discovered. The sailor, Mehemet, he who figured in the scene of the oath at Palamos, entered in a melancholy mood the enclosure where we were together, and made us understand that the cries of "Roumi!" vociferated under these circumstances, were equivalent to a sentence of death. "Wait," said he; "a means of saving you has occurred to me." Mehemet entered some moments afterwards, told us that his means had succeeded, and invited me to join the Kabyls, who were going to say prayers.

I accordingly went out, and prostrated myself towards the East. I imitated minutely the gestures which I saw made around me, pronouncing the sacred words,—*La elah il Allah! oua Mahommed raçoul Allah!* It was the scene of Mamamouchi of the "Bourgeois Gentilhomme," which I had so often seen acted by Dugazon, — with this one difference, that this time it did not make me laugh. I was, however, ignorant of the consequences it might have brought upon me on my arrival at Algiers. After having made the profession of faith before Mahomedans — *There is but one God, and Mahomet is his prophet*, if I had been informed against to the mufti, I must inevitably have become Mussulman, and they would not have allowed me to go out of the Regency.

I must not forget to relate by what means Mehemet had saved us from inevitable death. "You have guessed rightly," said he to the Kabyls; "there are two Christians in the caravansary, but they are Mahomedans at heart, and are going to Algiers to be adopted by the mufti into our holy religion. You will not doubt this when I tell you that I was myself a slave to some Christians,

and that they redeemed me with their money." "In cha Allah!' they exclaimed with one voice. And it was then that the scene took place which I have just described.

We arrived in sight of Algiers the 25th December, 1808. We took leave of the Arab owners of our mules, who walked on foot by the side of us, and we spurred them on, in order to reach the town before the closing of the gates. On our arrival, we learnt that the Dey, to whom we owed our first deliverance, had been beheaded. The guard of the palace before which we passed, stopped us and questioned us as to whence we came. We replied that we came from Bougie by land. "It is not possible!" exclaimed all the janissaries at once; "the Dey himself would not venture to undertake such a journey!" "We acknowledge that we have committed a great imprudence; that we would not undertake to recommence the journey for millions; but the fact that we have just declared is the strict truth."

Arrived at the consular house, we were, as on the first occasion, very cordially welcomed. We received a visit from a dragoman sent by the Dey, who asked whether we persisted in maintaining that Bougie had been our point of departure, and not Cape Matifou, or some neighbouring part. We again affirmed the truth of our recital; it was confirmed, the next day, on the arrival of the proprietors of our mules.

At Palamos, during the various interviews which I had with the dowager Duchess of Orleans, one circumstance had particularly affected me. The Princess spoke to me unceasingly of the wish she had to go and rejoin one of her sons, whom she believed to be alive, but of whose death I had been informed by a person belonging to her household. Hence I was anxious to do all that lay in my power to mitigate a sorrow which she must experience before long.

At the moment when I quitted Spain for Marseilles, the Duchess confided to me two letters which I was to forward in safety to their addresses. One was destined for the Empress-mother of Russia, the other for the Empress of Austria.

Scarcely had I arrived at Algiers, when I mentioned these two letters to M. Dubois Thainville, and begged him to send them to France by the first opportunity. "I shall do nothing of the sort," he at once answered me. "Do you know that you have behaved in this affair like a young inexperienced man, or, to speak out, like a blunderer? I am surprised that you did not comprehend that the Emperor, with his pettish spirit, might take this much amiss, and consider you, according to the contents of the two letters, as the promoter of an intrigue in favour of the exiled family of the Bourbons." Thus the paternal advice of the

French Consul taught me that in all that regards politics, however nearly or remotely, one cannot give himself up without danger to the dictates of the heart and the reason.

I enclosed my two letters in an envelope bearing the address of a trustworthy person, and gave them into the hands of a corsair, who, after touching at Algiers, would proceed to France. I have never known whether they reached their destination.

The reigning Dey, successor to the beheaded Dey, had formerly filled the humble office of "*épileur*"* of dead bodies in the mosques. He governed the Regency with much gentleness, occupying himself with little but his harem. This disgusted those who had raised him to this eminent post, and they resolved upon getting rid of him. We became aware of the danger which menaced him, by seeing the courts and vestibules of the consular house full, according to the custom under such circumstances, of Jews, carrying with them whatever they had of most value. It was a rule at Algiers, that all that happened in the interval comprised between the death of a Dey and the installation of his successor, could not be followed up by justice, and must remain unpunished. One can imagine, then, why the children of Moses should seek safety in the consular houses, the European inhabitants of which had the courage to arm themselves for self-defence as soon as the danger was apparent, and who, moreover, had a janissary to guard them.

Whilst the unfortunate Dey "épileur" was being conducted towards the place where he was to be strangled, he heard the cannon which announced his death and the installation of his successor. "They are in great haste," said he; "what will you gain by carrying matters to extremities? Send me to the Levant; I promise you never to return. What have you to reproach me with?" "With nothing," answered his escort, "but your insignificance. However, a man cannot live as a mere private man, after having been Dey of Algiers." And the unfortunate man perished by the rope.

The communication by sea between Bougie and Algiers was not so difficult, even with the "*sandalas*," as the Caïd of the former town wished to assure me. Captain Spiro had the cases landed, which belonged to me. The Caïd sought to discover what they contained; and, having perceived through a chink something yellowish, he hastened to send the news to the Dey, that the Frenchmen who had come to Algiers by land had among their baggage cases filled with zechins, destined to revolutionise

* An "*épileur*" is a person who removes superfluous hairs. We have been unable to ascertain what office of this kind is performed in Mohamedan funerals.

the Kabylie. They immediately had these cases forwarded to Algiers, and at their opening, before the Minister of Naval Affairs, all the phantasmagoria of zechins, of treasure, of revolution, disappeared at the sight of the stands and the limbs of several repeating circles in copper.

We are now going to sojourn several months in Algiers. I will take advantage of this to put together some details of manners which may be interesting as the picture of a state of things anterior to that of the occupation of the Regency by the French. This occupation, it must be remarked, has already fundamentally altered the manners and the habits of the Algerine population.

I am about to report a curious fact, and one which shows that politics, which insinuate themselves and bring discord into the bosom of the most united families, had succeeded, strange to say, in penetrating as far as the galley-slaves' prison at Algiers. The slaves belonged to three nations: there were in 1809 in this prison, Portuguese, Neapolitans, and Sicilians; among these two latter classes were counted partisans of Murat and those of Ferdinand of Naples. One day, at the beginning of the year, a dragoman came in the name of the Dey to beg M. Dubois Thainville to go without delay to the prison, where the friends of the French and their adversaries had involved themselves in a furious combat; and already several had fallen. The weapon with which they struck each other was the heavy long chain attached to their legs.

Each Consul, as I said above, had a janissary placed with him as his guard: the one belonging to the French Consul was a Candiote; he had been surnamed *the Terror.* Whenever some news unfavourable to France was announced in the cafés, he came to the Consulate to inform himself as to the reality of the fact; and when we told him that the other janissaries had propagated false news, he returned to them, and there, yatagan in hand, he declared himself ready to enter the lists in combat against those who should still maintain the truth of the news. As these continual threats might endanger him (for they had no support beyond his mere animal courage), we had wished to render him expert in the handling of arms by giving him some lessons in fencing; but he could not endure the idea that Christians should touch him at every turn with foils; he therefore proposed to substitute for the simulated duel a real combat with the yatagan.

One may gain an exact idea of this savage nature when I mention that, having one day heard a pistol-shot, the sound of which proceeded from his room, people ran, and found him bathed in his blood; he had just shot off a ball into his arm to cure himself of a rheumatic pain.

Seeing with what facility the Deys disappeared, I said one day to our janissary, "With this prospect before your eyes, would you consent to become Dey?" "Yes, doubtless," answered he. "You seem to count as nothing the pleasure of doing all that one likes, if only even for a single day!"

When we wished to take a turn in the town of Algiers, we generally took care to be escorted by the janissary attached to the consular house; it was the only means of escaping insults, affronts, and even acts of violence. I have just said it was the only means. I made a mistake; there was one other; that was, to go in the company of a French "lazarist" of seventy years of age, and whose name, if my memory serves me, was Father Joshua; he had lived in this country for half a century. This man, of exemplary virtue, had devoted himself with admirable self-denial to the service of the slaves of the Regency, and had divested himself of all considerations of nationality:—the Portuguese, Neapolitans, Sicilians, all were equally his brethren.

In the times of plague he was seen day and night carrying eager help to the Mussulmans; thus, his virtue had conquered even religious hatreds; and wherever he passed, he and the persons who might accompany him received from multitudes of the people, from the janissaries, and even from the officials of the mosques, the most respectful salutations.

During our long hours of sailing on board the Algerine vessel, and our compulsory stay in the prisons at Rosas, and on the hulk at Palamos, I gathered some ideas as to the interior life of the Moors or the Coulouglous, which, even now when Algiers has fallen under the dominion of France, would perhaps be yet worth preserving. I shall, however, confine myself to recounting, nearly word for word, a conversation which I had with Raïs Braham, whose father was a "*Turc fin*," that is to say, a Turk born in the Levant.

"How is it that you consent," said I to him, "to marry a young girl whom you have never seen, and find in her, perhaps, an excessively ugly woman, instead of the beauty whom you had fancied to yourself?"

"We never marry without having obtained information from the women who serve in the capacity of servants at the public baths. The Jewesses are moreover, in these cases, very useful go-betweens."

"How many legitimate wives have you?"

"I have four, that is to say, the number authorised by the Koran."

"Do they live together on a good understanding?"

"Ah, sir, my house is a hell. I never enter it without finding

them at the step of the door, or at the bottom of the stairs; then, each wants to be the first to make me listen to the complaints which she has to bring against her companions. I am about to utter blasphemy, but I think that our holy religion ought to prohibit a plurality of wives to those who are not rich enough to give to each a separate habitation."

"But since the Koran allows you to repudiate even legitimate wives, why do you not send back three of them to their parents?"

"Why? because that would ruin me. On the day of the marriage the father of the young woman to be married stipulates for a dowry, and the half of it is paid. The other half may be exacted the day that the woman is repudiated. It would then be three half dowries that I should have to pay if I sent back three of my wives. I ought, however, to rectify one inaccuracy in what I said just now, that my four wives had never agreed together. Once, they were agreed among themselves in the feeling of a common hatred. In going through the market I had bought a young negress. In the evening, when I retired to rest, I perceived that my wives had prepared no bed for her, and that the unfortunate girl was extended on the ground. I rolled up my trowsers and laid them under her head as a kind of pillow. In the morning the distracting cries of the poor slave made me run to her, and I found her nearly sinking under the blows of my four wives; for once they understood each other marvellously well."

In February 1809, the new Dey, the successor of the "épileur," a short time after having entered on his functions, claimed from two to three hundred thousand francs,—I do not remember exactly the sum,—which he pretended was due to him from the French Government. M. Dubois Thainville answered that he had received the Emperor's orders not to pay one centime.

The Dey was furious, and decided upon declaring war against us. A declaration of war at Algiers used to be immediately followed by putting all the persons of other nations into prison. This time matters were not pushed to this extreme limit. Our names might be figuring on the list of the slaves of the Regency; but in fact, so far as I was concerned, I remained free in the consular house. By means of a pecuniary guarantee, contracted with the Swedish Consul, M. Norderling, I was even permitted to live at his country house, situated near the Emperor's fort.

The most insignificant event was sufficient to modify the ideas of these barbarians. I had come into the town one day, and was seated at table at M. Dubois Thainville's, when the English Consul, Mr. Blankley, arrived in great haste, announcing to our Consul the entrance into the port of a French prize. "I never

will uselessly add," said he, generously, "to the severities of war; I came to announce to you, my colleague, that I will give up your prisoners on a receipt which will insure me the deliverance of an equal number of Englishmen detained in France." "I thank you," answered M. Dubois Thainville; "but I do not the less deplore this event that it will retard, indefinitely, perhaps, the settlement of the account in which I am engaged with the Dey."

During this conversation, armed with a telescope, I was looking through the window of the dining-room, trying to persuade myself at least that the captured vessel was not one of much importance. But one must yield to evidence. It was pierced for a great number of guns. All at once, the wind having displayed the flags, I perceived with surprise the French flag over the English flag. I communicated what I observed to Mr. Blankley. He answered immediately, "You do not surely pretend to observe better with your bad telescope than I did with my *Dollond?*"

"And you cannot pretend," said I to him in *my* turn, "to see better than an astronomer by profession? I am sure of my fact. I beg M. Thainville's permission, and will go this instant to visit this mysterious prize."

In short, I went there; and this is what I learnt:—

General Duhesme, Governor of Barcelona, wishing to rid himself of the most ill-disciplined portion of his garrison, formed the principal part into the crew of a vessel, the command of which he gave to a lieutenant of Babastro, a celebrated corsair of the Mediterranean.

There were amongst these improvised seamen a hussar, a dragoon, two veterans, a miner with his long beard, &c. &c. The vessel, leaving Barcelona by night, escaped the English cruiser, and got to the entrance of Port Mahon. An English "lettre de marque" was coming out of the port. The crew of the French vessel boarded her; and a furious combat on the deck ensued, in which the French got the upper hand. It was this "lettre de marque" which had now arrived at Algiers.

Invested with full power by M. Dubois Thainville, I announced to the prisoners that they were about to be immediately given up to their Consul. I respected even the trick of the captain, who, wounded by several sabre-cuts, had contrived to cover up his head with his principal flag. I re-assured his wife; but my chief care was especially devoted to a passenger whom I saw with one arm amputated.

"Where is the surgeon," I said to him, "who operated on you?"

"It was not our surgeon," he answered. "He basely fled with a part of the crew, and saved himself on land."

"Who, then, cut off your arm?"

"It was the hussar whom you see here."

"Unhappy man!" I exclaimed; "what could lead you, when it was not your profession, to perform this operation?"

"The pressing request of the wounded man. His arm had already swollen to an enormous size. He wanted some one to cut it off for him with a blow of a hatchet. I told him that in Egypt, when I was in hospital, I had seen several amputations made; that I would imitate what I had seen, and might perhaps succeed. That at any rate it would be better than the blow of a hatchet. All was agreed; I armed myself with the carpenter's saw; and the operation was done."

I went off immediately to the American consul, to claim the assistance of the only surgeon worthy of confidence who was then in Algiers. M. Triplet — I think I recollect that that was the name of the man of the distinguished art whose aid I invoked — came at once on board the vessel, examined the dressing of the wound, and declared, to my very lively satisfaction, that all was going on well, and that the Englishman would survive his horrible injury.

The same day we had the wounded men carried on litters to Mr. Blankley's house; this operation, executed with somewhat of ceremony, modified, though slightly, the feelings of the Dey in our favour, and his sentiments became yet more favourable towards us in consequence of another maritime occurrence, although a very insignificant one.

One day a corvette was seen in the horizon armed with a very great number of guns, and shaping her way towards the port of Algiers; there appeared immediately after an English brig of war, in full sail; a combat was therefore expected, and all the terraces of the town were covered with spectators; the brig appeared to be the best sailer, and seemed to us likely to reach the corvette, but the latter tacked about, and seemed desirous to engage in battle; the English vessel fled before her; the corvette tacked about a second time, and again directed her course towards Algiers, where, one would have supposed, she had some special mission to execute. The brig, in her turn now changed her course, but held herself constantly beyond the reach of shot from the corvette; at last the two vessels arrived in succession in the port, and cast anchor, to the lively disappointment of the Algerine population, who had hoped to be present without danger at a maritime combat between the "Christian dogs," belonging to two nations equally detested in a religious point of view; but shouts of laughter could not be repressed when it was seen that the corvette was a merchant vessel, and that she was only armed with wooden imitations of cannon. It was said in the town that the English sailors were

furious, and had been on the point of mutiny against their too prudent captain.

I have very little to tell in favour of the Algerines: hence I must do an act of justice by mentioning, that the corvette departed the next day for the Antilles, her destination, and that the brig was not permitted to set sail until the next day but one.

Bakri often came to the French Consulate to talk of our affairs with M. Dubois Thainville: "What can you want?" said the latter, "you are an Algerine; you will be the first victim of the Dey's obstinacy. I have already written to Livorno that your families and your goods are to be seized. When the vessels laden with cotton, which you have in this port, arrive at Marseilles, they will be immediately confiscated; it is for you to judge whether it would not better suit you to pay the sum which the Dey claims, than to expose yourself to tenfold and certain loss."

Such reasoning was unanswerable; and whatever it might cost him, Bakri decided on paying the sum that was demanded of France.

Permission to depart was immediately granted to us; I embarked the 21st of June, 1809, on board a vessel in which M. Dubois Thainville and his family were passengers.

The evening before our departure from Algiers, a corsair deposited at the consul's the Majorcan mail, which he had taken from a vessel which he had captured. It was a complete collection of the letters which the inhabitants of the Baléares had been writing to their friends on the Continent.

"Look here," said M. Dubois Thainville to me, "here is something to amuse you during the voyage, — you who generally keep your room from sea-sickness, — break the seals and read all these letters, and see whether they contain any accounts by which we might profit how to aid the unhappy soldiers who are dying of misery and despair in the little island of Cabrera."

Scarcely had we arrived on board the vessel, when I set myself to the work, and acted without scruple or remorse the part of an official of the black chamber, with this sole difference, that the letters were unsealed without taking any precautions. I found amongst them several dispatches, in which Admiral Collingwood signified to the Spanish Government the ease with which the prisoners might be delivered. Immediately on our arrival at Marseilles these letters were sent to the minister of naval affairs, who, I believe, did not pay much attention to them.

I knew almost every one at Palma, the capital of Majorca. I leave it to be imagined with what curiosity I read the missives in which the beautiful ladies of the town expressed their hatred against *los malditos cavachios* (French), whose presence in Spain

had rendered necessary the departure for the Continent of a magnificent regiment of hussars; how many persons might I not have embroiled, if under a mask I had found myself with them at the opera ball!

Many of the letters made mention of me, and were particularly interesting to me; I was sure in this instance there was nothing to constrain the frankness of those who had written them. It is an advantage which few people can boast having enjoyed to the same degree.

The vessel in which I was, although laden with bales of cotton, had some corsair papers of the Regency, and was the reputed escort of three richly laden merchant vessels which were going to France.

We were off Marseilles on the 1st of July, when an English frigate came to stop our passage: "I will not take you," said the English captain; "but you will go towards the Hyères Islands, and Admiral Collingwood will decide on your fate."

"I have received," answered the Barbary captain, an express commission to conduct these vessels to Marseilles, and I will execute it."

"You, individually, can do what may seem to you best," answered the Englishman; "as to the merchant vessels under your escort, they will be, I repeat to you, taken to Admiral Collingwood." And he immediately gave orders to those vessels to set sail to the East.

The frigate had already gone a little distance when she perceived that we were steering towards Marseilles. Having then learnt from the crews of the merchant vessels that we were ourselves laden with cotton, she tacked about to seize us.

She was very near reaching us, when we were enabled to enter the port of the little island of Pomègue. In the night she put her boats to sea to try to carry us off; but the enterprise was too perilous, and she did not dare attempt it.

The next morning, 2nd of July, 1809, I disembarked at the lazaretto.

At the present day they go from Algiers to Marseilles in four days: it had taken me eleven months to make the same voyage. It is true that here and there I had made involuntary sojourns.

My letters sent from the lazaretto at Marseilles were considered by my relatives and friends as certificates of resurrection, they having for a long time past supposed me dead. A great geometer had even proposed to the Bureau of Longitude no longer to pay my allowance to my authorised representative; which appears the more cruel inasmuch as this representative was my father.

The first letter which I received from Paris was full of sym-

pathy and congratulations on the termination of my laborious and perilous adventures; it was from a man already in possession of an European reputation, but whom I had never seen: M. de Humboldt, after what he had heard of my misfortunes, offered me his friendship. Such was the first origin of a connection which dates from nearly forty-two years back, without a single cloud ever having troubled it.

M. Dubois Thainville had numerous acquaintances in Marseilles; his wife was a native of that town, and her family resided there. They received, therefore, both of them, numerous visits in the parlour of the lazaretto. The bell which summoned them, for me alone was dumb; and I remained as solitary and forsaken, at the gates of a town peopled with a hundred thousand of my country-men, as if I had been in the heart of Africa. One day, however, the parlour-bell rang three times (the number of times corresponding to the number of my room); I thought it must be a mistake. I did not, however, allow this to appear. I traversed proudly under the escort of my guard of health the long space which separates the lazaretto, properly so called, from the parlour; and there I found, with very lively satisfaction, M. Pons, the director of the Observatory at Marseilles, and the most celebrated discoverer of comets of whom the annals of Astronomy have ever had to register the success.

At any time a visit from the excellent M. Pons, whom I have since seen director of the Observatory at Florence, would have been very agreeable to me; but, during my quarantine, I felt it unappreciably valuable. It proved to me that I had returned to my native soil.

Two or three days before our admission to freedom, we experienced a loss which was deeply felt by each of us. To pass away the heavy time of a severe quarantine, the little Algerine colony was in the habit of going to an enclosure near the lazaretto, where a very beautiful gazelle, belonging to M. Dubois Thainville, was confined; she bounded about there in full liberty with a grace which excited our admiration. One of us endeavoured to stop this elegant animal in her course; he seized her unluckily by the leg, and broke it. We all ran, but only, alas! to witness a scene which excited the deepest emotion in us.

The gazelle, lying on her side, raised her head sadly; her beautiful eyes (the eyes of a gazelle!) shed torrents of tears; no cry of complaint escaped her mouth; she produced that effect upon us which is always felt when a person who is suddenly struck by an irreparable misfortune, resigns himself to it, and shows his profound anguish only by silent tears.

Having ended my quarantine, I went at once to Perpignan, to

the bosom of my family, where my mother, the most excellent and pious of women, caused numerous masses to be said to celebrate my return, as she had done before to pray for the repose of my soul, when she thought that I had fallen under the daggers of the Spaniards. But I soon quitted my native town to return to Paris; and I deposited at the Bureau of Longitude and the Academy of Sciences my observations, which I had succeeded in preserving amidst the perils and tribulations of my long campaign.

A few days after my arrival, on the 18th of September, 1809, I was nominated an academician in the place of Lalande. There were fifty-two voters; I obtained forty-seven voices, M. Poisson four, and M. Nouet one. I was then twenty-three years of age.

A nomination made with such a majority would appear, at first sight, as if it could give rise to no serious difficulties; but it proved otherwise. The intervention of M. de Laplace, before the day of ballot, was active and incessant to have my admission postponed until the time when a vacancy, occurring in the geometry section, might enable the learned assembly to nominate M. Poisson at the same time as me. The author of the *Mécanique Céleste* had vowed to the young geometer an unbounded attachment, completely justified, certainly, by the beautiful researches which science already owed to him. M. de Laplace could not support the idea that a young astronomer, younger by five years than M. Poisson, a pupil, in the presence of his professor at the Polytechnic School, should become an academician before him. He proposed to me, therefore, to write to the Academy that I would not stand for election until there should be a second place to give to Poisson. I answered by a formal refusal, and giving my reasons in these terms: "I care little to be nominated at this moment. I have decided upon leaving shortly with M. de Humboldt for Thibet. In those savage regions the title of member of the Institute will not smooth the difficulties which we shall have to encounter. But I would not be guilty of any rudeness towards the Academy. If they were to receive the declaration for which I am asked, would not the savans who compose this illustrious body have a right to say to me: 'How are you certain that we have thought of you? You refuse what has not yet been offered to you.'"

On seeing my firm resolution not to lend myself to the inconsiderate course which he had advised me to follow, M. de Laplace went to work in another way; he maintained that I had not sufficient distinction for admission into the Academy. I do not pretend that, at the age of three-and-twenty, my scientific attainments were very considerable, if estimated in an *absolute* manner; but

when I judged by *comparison*, I regained courage, especially on considering that the three last years of my life had been consecrated to the measurement of an arc of the meridian in a foreign country; that they were passed amid the storms of the war with Spain; often enough in dungeons, or, what was yet worse, in the mountains of Kabylia, and at Algiers, at that time a very dangerous residence.

Here is, therefore, my statement of accounts for that epoch. I make it over to the impartial appreciation of the reader.

On leaving the Polytechnic School, I had made, in conjunction with M. Biot, an extensive and very minute research on the determination of the coefficient of the tables of atmospheric refraction.

We had also measured the refraction of different gases, which, up to that time, had not been attempted.

A determination, more exact than had been previously obtained, of the relation of the weight of air to the weight of mercury, had furnished a direct value of the coefficient of the barometrical formula which served for the calculation of the heights.

I had contributed, in a regular and very assiduous manner, during nearly two years, to the observations which were made day and night with the transit telescope and with the mural quadrant at the Paris Observatory.

I had undertaken, in conjunction with M. Bouvard, the observations relating to the verification of the laws of the moon's libration. All the calculations were prepared; it only remained for me to put the numbers into the formulæ, when I was, by order of the Bureau of Longitude, obliged to leave Paris for Spain. I had observed various comets, and calculated their orbits. I had, in concert with M. Bouvard, calculated, according to Laplace's formula, the table of refraction which has been published in the *Recueil des Tables* of the Bureau of Longitude, and in the *Connaissance des Temps.* A research on the velocity of light, made with a prism placed before the object end of the telescope of the mural circle, had proved that the same tables of refraction might serve for the sun and all the stars.

Finally, I had just terminated, under very difficult circumstances, the grandest triangulation which had ever been achieved, to prolong the meridian line from France as far as the island of Formentera.

M. de Laplace, without denying the importance and utility of these labours and these researches, saw in them nothing more than indications of promise; M. Lagrange then said to him explicitly: —

"Even you, M. de Laplace, when you entered the Academy,

had done nothing brilliant; you only gave promise. Your grand discoveries did not come till afterwards."

Lagrange was the only man in Europe who could with authority address such an observation to him.

M. de Laplace did not reply upon the ground of the personal question, but he added,—"I maintain that it is useful to young savans to hold out the position of member of the Institute as a future recompence, to excite their zeal."

"You resemble," replied M. Hallé, "the driver of the hackney coach, who, to excite his horses to a gallop, tied a bundle of hay at the end of his carriage pole; the poor horses redoubled their efforts, and the bundle of hay always flew on before them. After all, his plan made them fall off, and soon after brought on their death."

Delambre, Legendre, Biot, insisted on the devotion, and what they termed the courage, with which I had combated arduous difficulties, whether in carrying on the observations, or in saving the instruments and the results already obtained. They drew an animated picture of the dangers I had undergone. M. de Laplace ended by yielding when he saw that all the most eminent men of the Academy had taken me under their patronage, and on the day of the election he gave me his vote. It would be, I must own, a subject of regret with me even to this day, after a lapse of forty-two years, if I had become member of the Institute without having obtained the vote of the author of the *Mécanique Céleste.*

The Members of the Institute were always presented to the Emperor after he had confirmed their nominations. On the appointed day, in company with the presidents, with the secretaries of the four classes, and with the academicians who had special publications to offer to the Chief of the State, they assembled in one of the saloons of the Tuileries. When the Emperor returned from mass, he held a kind of review of these savans, these artists, these literary men, in green uniform.

I must own that the spectacle which I witnessed on the day of my presentation did not edify me. I even experienced real displeasure in seeing the anxiety evinced by members of the Institute to be noticed.

"You are very young," said Napoleon to me on coming near me; and without waiting for a flattering reply, which it would not have been difficult to find, he added,—"What is your name?" And my neighbour on the right, not leaving me time to answer the simple enough question just addressed to me, hastened to say,—

"*His* name is Arago."

"What science do you cultivate?"

My neighbour on the left immediately replied,—

"*He* cultivates astronomy."

"What have you done?"

My neighbour on the right, jealous of my left hand neighbour for having encroached on his rights at the second question, now hastened to reply, and said,—

"*He* has just been measuring the line of the meridian in Spain."

The Emperor imagining doubtless that he had before him either a dumb man or an imbecile, passed on to another member of the Institute. This one was not a novice, but a naturalist well known through his beautiful and important discoveries; it was M. Lamarck. The old man presented a book to Napoleon.

"What is that?" said the latter, "it is your absurd *meteorology*, in which you rival Matthieu Laensberg. It is this 'annuaire' which dishonours your old age. Do something in Natural History, and I should receive your productions with pleasure. As to this volume, I only take it in consideration of your white hair. Here!" And he passed the book to an aide-de-camp.

Poor M. Lamarck, who, at the end of each sharp and insulting sentence of the Emperor, tried in vain to say, "It is a work on Natural History which I present to you," was weak enough to fall into tears.

The Emperor immediately afterwards met with a more energetic antagonist in the person of M. Lanjuinais. The latter had advanced, book in hand. Napoleon said to him, sneeringly:—

"The entire Senate, then, is to merge in the Institute?" "Sire," replied Lanjuinais, "it is the body of the state to which most time is left for occupying itself with literature."

The Emperor, displeased at this answer, at once quitted the civil uniforms, and busied himself among the great epaulettes which filled the room.

Immediately after my nomination, I was exposed to strange annoyances on the part of the military authorities. I had left for Spain, still holding the title of pupil of the Polytechnic School. My name could not remain on the books more than four years; consequently I had been enjoined to return to France to go through the examinations necessary on quitting the school. But in the meantime Lalande died, and thus a place in the Bureau of Longitude became vacant. I was named assistant-astronomer. These places were submitted to the nomination of the Emperor. M. Lacuée, Director of the Conscription, thought that, through

this latter circumstance, the law would be satisfied, and I was authorised to continue my operations.

M. Matthieu Dumas, who succeeded him, looked at the question from an entirely different point of view; he enjoined me either to furnish a substitute, or else to set off myself with the contingent of the twelfth arrondissement of Paris.

All my remonstrances and those of my friends having been fruitless, I announced to the honourable General that I should present myself in the Place de l'Estrapade, whence the conscripts had to depart, in the costume of a member of the Institute; and that thus I should march on foot through the city of Paris. General Matthieu Dumas was alarmed at the effect which this scene would produce on the Emperor, himself a member of the Institute, and hastened, under fear of my threat, to confirm the decision of General Lacuée.

In the year 1809, I was chosen by the "conseil du perfectionnement" of the Polytechnic School, to succeed M. Monge, in his chair of Analysis applied to Geometry. The circumstances attending that nomination have remained a secret; I seize the first opportunity which offers itself to me to make them known.

M. Monge took the trouble to come to me one day, at the Observatory, to ask me to succeed him. I declined this honour, because of a proposed journey which I was going to make into Central Asia with M. de Humboldt. "You will certainly not set off for some months to come," said the illustrious geometer; "you could, therefore, take my place temporarily." "Your proposal," I replied, "flatters me infinitely; but I do not know whether I ought to accept it. I have never read your great work on partial differential equations; I do not, therefore, feel certain that I should be competent to give lessons to the pupils of the Polytechnic School on such a difficult theory." "Try," said he, "and you will find that that theory is clearer than it is generally supposed to be." Accordingly, I did try; and M. Monge's opinion appeared to me to be well founded.

The public could not comprehend, at that time, how it was that the benevolent M. Monge obstinately refused to confide the delivery of his course to M. Binet (a private teacher under him), whose zeal was well known. It is this motive which I am going to reveal.

There was then in the "Bois de Boulogne" a residence named the *Grey House*, where there assembled round M. Coessin, the high-priest of a new religion, a number of adepts, such as Lesueur, the musician, Colin, private teacher of chemistry at the school, M. Binet, &c. A report from the prefect of police had signified

the Emperor that the frequenters of the Grey House were con-ected with the Society of Jesuits. The Emperor was uneasy nd irritated at this. "Well," said he to M. Monge, "there are our dear pupils become disciples of Loyola!" And on Monge's enial, "You deny it," answered the Emperor; "well, then, know nat the private teacher of your course is in that clique." Every ne can understand that after such a remark, Monge could not onsent to being succeeded by M. Binet.

Having entered the Academy, young, ardent, and impassioned, took much greater part in the nominations than may have been uitable for my position and my time of life. Arrived at an epoch f life whence I examine retrospectively all my actions with calm-ess and impartiality, I can render this amount of justice to myself, hat, excepting in three or four instances, my vote and interest vere always in favour of the most deserving candidate, and more han once I succeeded in preventing the Academy from making a leplorable choice. Who could blame me for having maintained vith energy the election of Malus, considering that his competitor, M. Girard, unknown as a physicist, obtained twenty-two votes out of fifty-three, and that an addition of five votes would have given him the victory over the savant who had just discovered the phenomenon of polarisation by reflection, over the savant whom Europe would have named by acclamation? The same remarks are applicable to the nomination of Poisson, who would have failed against this same M. Girard if four votes had been otherwise given. Does not this suffice to justify the unusual ardour of my conduct? Although in a third trial the majority of the Academy was decided in favour of the same engineer, I cannot regret that I supported up to the last moment with conviction and warmth the election of his competitor, M. Dulong.

I do not suppose that, in the scientific world, any one will be disposed to blame me for having preferred M. Liouville to M. de Pontécoulant.

Sometimes it happened that the Government wished to influence the choice of the Academy: with a strong sense of my rights I invariably resisted all dictation. Once this resistance acted unfortunately on one of my friends — the venerable Legendre; as to myself, I had prepared myself beforehand for all the persecutions of which I could be made the object. Having received from the Minister of the Interior an invitation to vote for M. Binet against M. Navier on the occurrence of a vacant place in the section of mechanics, Legendre nobly answered that he would vote according to his soul and his conscience. He was immediately deprived of a pension which his great age and his long

services rendered due to him. The *protégé* of the authorities failed; and, at the time, this result was attributed to the activity with which I enlightened the members of the Academy as to the impropriety of the Minister's proceedings.

On another occasion the King wished the Academy to name Dupuytren, the eminent surgeon, but whose character at the time lay under grave imputations. Dupuytren was nominated, but several blanks protested against the interference of the authorities in academic elections.

I said above that I had saved the Academy from some deplorable choices; I will only cite a single instance, on which occasion I had the sorrow of finding myself in opposition to M. de Laplace. The illustrious geometer wished a vacant place in the astronomical section to be granted to M. Nicollet,— a man without talent, and, moreover, suspected of misdeeds which reflected on his honour in the most serious degree. At the close of a contest, which I maintained undisguisedly, notwithstanding the danger which might follow from thus braving the powerful protectors of M. Nicollet, the Academy proceeded to the ballot; the respected M. Damoiseau, whose election I had supported, obtained forty-five votes out of forty-eight. Thus M. Nicollet had collected but three.

"I see," said M. de Laplace to me, "that it is useless to struggle against young people; I acknowledge that the man who is called the *great elector* of the Academy is more powerful than I am."

"No," replied I; "M. Arago can only succeed in counterbalancing the opinion justly preponderating for M. de Laplace, when the right is found to be without possible contradiction on his side."

A short time afterwards M. Nicollet had run away to America, and the Bureau of Longitude had a warrant passed to expel him ignominiously from its bosom.

I would warn those savans, who, having early entered the Academy, might be tempted to imitate my example, to expect nothing beyond the satisfaction of their conscience. I warn them, with a knowledge of the case, that gratitude will almost always be found wanting.

The elected academician, whose merits you have sometimes exalted beyond measure, pretends that you have done no more than justice to him; that you have only fulfilled a duty, and that he therefore owes you no thanks.

Delambre died the 19th August, 1822. After the necessary delay, they proceeded to fill his place. The situation of Perpetual Secretary is not one which can long be left vacant. The

Academy named a commission to present it with candidates; it was composed of Messrs. de Laplace, Arago, Legendre, Rossel, Prony, and Lacroix. The list presented was composed of the names of Messrs. Biot, Fourier, and Arago. It is not necessary for me to say with what obstinacy I opposed the inscription of my name on this list; I was compelled to give way to the will of my colleagues, but I seized the first opportunity of declaring publicly that I had neither the expectation nor the wish to obtain a single vote; that, moreover, I had on my hands already as much work as I could get through; that in this respect M. Biot was in the same position; and that, in short, I should vote for the nomination of M. Fourier.

It was supposed, but I dare not flatter myself that it was the fact, that my declaration exercised a certain influence on the result of the ballot. The result was as follows: M. Fourier received thirty-eight votes, and M. Biot ten. In a case of this nature each man carefully conceals his vote, in order not to run the risk of future disagreement with him who may be invested with the authority which the Academy gives to the perpetual secretary. I do not know whether I shall be pardoned if I recount an incident which amused the Academy at the time.

M. de Laplace, at the moment of voting, took two plain pieces of paper; his neighbour was guilty of the indiscretion of looking, and saw distinctly that the illustrious geometer wrote the name of Fourier on both of them. After quietly folding them up, M. de Laplace put the papers into his hat, shook it, and said to this same curious neighbour: "You see, I have written two papers; I am going to tear up one, I shall put the other into the urn; I shall thus be myself ignorant for which of the two candidates I have voted."

All went on as the celebrated academician had said; only that every one knew with certainty that his vote had been for Fourier; and "the calculation of probabilities" was in no way necessary for arriving at this result.

After having fulfilled the duties of secretary with much distinction, but not without some feebleness and negligence in consequence of his bad health, Fourier died the 16th of May, 1830. I declined several times the honour which the Academy appeared willing to do me, in naming me to succeed him. I believed, without false modesty, that I had not the qualities necessary to fill this important place suitably. When thirty-nine out of forty-four voters had appointed me, it was quite time that I should give in to an opinion so flattering and so plainly expressed. On the 7th of June, 1830, I, therefore, became perpetual secretary of the

Academy for the Mathematical Sciences; but, conformably to the plea of an accumulation of offices, which I had used as an argument to support, in November, 1822, the election of M. Fourier, I declared that I should give in my resignation of the Professorship in the Polytechnic School. Neither the solicitations of Marshal Soult, the Minister of War, nor those of the most eminent members of the Academy, could avail in persuading me to renounce this resolution.

BAILLY.

BIOGRAPHY READ AT THE PUBLIC SITTING OF THE ACADEMY OF SCIENCES, THE 26TH OF FEBRUARY, 1844.

INTRODUCTION.

ENTLEMEN, — The learned man, illustrious in so many ways, hose life I am going to relate, was taken from France half a entury ago. I hasten to make this remark, so as thoroughly to now that I have selected this subject without being deterred by omplaints which I look upon as unjust and inapplicable. The lory of the members of the early Academy of Sciences is an ineritance for the present Academy. We must cherish it as we ould the glory of later days; we must hallow it with the same espect, we must devote to it the same worship: the word *preription* would here be synonymous with ingratitude.

If it had happened, Gentlemen, that amongst the academicians ho preceded us, a man, already illustrious by his labours, and, withut personal ambition, yet thrown, despite himself, into the midst of terrible revolution, exposed to a thousand unrestrained passions, ad cruelly disappeared in the political effervescence — oh! then, ny negligence, any delay in studying the facts would be inexusable; the honourable contemporaries of the victim would soon e no longer there to shed the light of their honest and impartial emory on obscure events; an existence devoted to the culvation of reason and of truth would come to be appreciated only om documents, on which, for my part, I would not blindly draw, ntil it shall be proved that, in revolutionary times, we can trust the uprightness of parties.

I felt in duty bound, Gentlemen, to give you a sketch of the eas that have led me to present to you a detailed account of the fe and labours of a member of the early Academy of Sciences. he biographies which will soon follow this, will show that the

studies I have undertaken respecting Carnot, Condorcet and Bailly, have not prevented me from attending seriously to our illustrious contemporaries.

To render them a loyal and truthful homage, is the first duty of the secretaries of the Academy, and I will religiously fulfil it; without binding myself, however, to observe a strict chronological order, or to follow the civil registers step by step.

Eulogies, said an ancient authority, should be deferred until we have lost the true measure of the dead. Then we could make giants of them without any one opposing us. On the contrary, I am of opinion that biographers, especially those of academicians, ought to make all possible haste, so that every one may be represented according to his true measure, and that well-informed people may have the opportunity of rectifying the mistakes which, notwithstanding every care, almost inevitably slip into this sort of composition. I regret that our former secretaries did not adopt this rule. By deferring from year to year to analyse the scientific and political life of Bailly with their scruples, and with their usual talents, they allowed time for inconsiderateness, prejudice, and passions of every kind, to impregnate our minds with a multitude of serious errors, which have added considerably to the difficulty of my task. When I was led to form very different opinions from those that are found spread through some of the most celebrated works, on the events of the great revolution of 1789, in which our fellow-academician took an active part, I could not be so conceited as to expect to be believed on my own word. To propound my opinions then was insufficient; I had also to combat those of the historians with whom I differed. This necessity has given to the biography that I am going to read an unusual length. I solicit the kind sympathy of the assembly on this point. I hope to obtain it, I acknowledge, when I consider that my task is to analyse before you the scientific and literary claims of an illustrious colleague, to depict the uniformly noble and patriotic conduct of the first President of the National Assembly; to follow the first Mayor of Paris in all the acts of an administration, the difficulties of which appeared to be above human strength to accompany the virtuous magistrate to the very scaffold, to unroll the mournful phases of the cruel martyrdom that he was made to undergo; to retrace, in a word, some of the greatest, some of the most terrible events of the French Revolution.

INFANCY OF BAILLY. — HIS YOUTH. — HIS LITERARY ESSAYS. — HIS MATHEMATICAL STUDIES.

John Sylvain Bailly was born at Paris in 1736. His parents were James Bailly and Cecilia Guichon.

The father of the future astronomer had charge of the king's pictures. This post had continued in the obscure but honest family of Bailly for upwards of a century.

Sylvain, while young, never quitted his paternal home. His mother would not be separated from him; it was not that she could give him the instruction required from masters in childhood, but a tenderness, allowed to run to the utmost extreme, entirely blinded her. Bailly then formed his own mind, under the eye of his parents. Nothing could be better, it seemed, than the boyhood of our brother academician, to verify the oft-repeated theory, touching the influence of imitation on the development of our faculties. Here, the result, attentively examined, would not by a great deal agree with the old hypothesis. I know not but, every thing considered, whether it would rather furnish powerful weapons to whoever would wish to maintain that, in its early habits, childhood rather seeks for contrasts.

James Bailly had an idle and light character; whilst young Sylvain from the beginning showed strong reasoning powers, and a passion for study.

The grown man felt in his own element while in noisy gaiety.

But the boy loved retirement.

To the father, solitude would have been fatal; for to him life consisted in motion, sallies, witty conversations, free and easy parties, the little gay suppers of those days.

The son, on the contrary, would remain alone and quite silent for whole days. His mind sufficed to itself; he never sought the fellowship of companions of his own age. Extreme steadiness was at once his habit and his taste.

The warder of the king's pictures drew remarkably well, but did not appear to have troubled himself much with the principles of art.

His son Sylvain studied those principles deeply, and to some purpose; he became a theoretic artist of the first class, but he never could either draw or paint even moderately well.

There are few young people who would not, at some time or other, have wished to escape from the scrutinizing eyes of their parents. The contrary was the case in Bailly's family, for James used sometimes to say to his friends or to his servants, "Do not mention this peccadillo to my son. Sylvain is worth more than I am; his morals are very strict. Under the most respectful exterior, I should perceive in his manner a censure which would grieve me. I wish to avoid his tacit reproaches, even when he does not say a word."

The two characters resembled each other only in one point—in

their taste for poetry, or perhaps we ought to say versification, but even here we shall perceive differences.

The father composed songs, little interludes, and farces that were acted at the *Italian Comedy;* but the son commenced at the age of sixteen by a serious work of time,—a tragedy.

This tragedy was entitled *Clothaire.* The subject, drawn from the early centuries of the French History, had led Bailly by a curious and touching coincidence to relate the tortures inflicted on a Mayor of Paris by a deluded and barbarous multitude. The work was modestly submitted to the actor Lanoue, who, although he bestowed flattering encouragement on Bailly, dissuaded him frankly from exposing *Clothaire* to the risk of a public representation. On the advice of the comedian-author, the young poet took *Iphygenia in Tauris* for the subject of his second composition. Such was his ardour, that by the end of three months, he had already written the last line of the fifth act of his new tragedy, and hastened to Passy, to solicit the opinion of the author of *Mahomet II.* This time Lanoue thought he perceived that his confiding young friend was not intended by nature for the drama, and he declared it to him without disguise. Bailly heard the fatal sentence with more resignation than could have been expected from a youth whose budding self-esteem received so violent a shock. He even threw his two tragedies immediately into the fire. Under similar circumstances, Fontenelle showed less docility in his youth. If the tragedy of *Aspar* also disappeared in the flames, it was not only in consequence of the criticism of a friend; for the author went so far as to call forth the noisy judgment of the pit.

Certainly no astronomer will regret that any opinions, either offhand or well digested, on the first literary productions of Bailly, contributed to throw him into the pursuit of science. Still, for the sake of principle, it seems just to protest against the praises given to the foresight of Lanoue, to the sureness of his judgment, to the excellence of his advice. What was it in fact? A lad of sixteen or seventeen years of age, composes two tolerable tragedies, and these essays are made irrevocably to decide on his future fate. We have then forgotten that Racine had already reached the age of twenty-two, when he first appeared, producing *Theagenes and Charicles,* and the *Inimical Brothers;* that Crébillon was nearly forty years of age when he composed a tragedy on *The Death of the Sons of Brutus,* of which not a single verse has been preserved; finally, that the two first comedies of Molière, *The three rival Doctors* and *The Schoolmaster,* are no longer known but by their titles. Let us recall to mind that reflection of Voltaire's: "It is very difficult to succeed before the age of thirty in a branch of

literature that requires a knowledge of the world and of the human heart."

A happy chance showed that the sciences might open an honourable and glorious path to the discouraged poet. M. de Moncaville offered to teach him mathematics, in exchange for drawing-lessons that his son received from the warder of the king's pictures. The proposal being accepted, the progress of Sylvain Bailly in these studies was rapid and brilliant.

BAILLY BECOMES THE PUPIL OF LACAILLE. — HE IS ASSOCIATED WITH HIM IN HIS ASTRONOMICAL LABOURS.

The mathematical student soon after had one of those providential meetings which decide a young man's future fate. Mademoiselle Lejeuneux cultivated painting. It was at the house of this female artist, known afterwards as Madame La Chenaye, that Lacaille saw Bailly. The attentive, serious, and modest demeanour of the student charmed the great astronomer. He showed it in a most unequivocal manner, by offering, though so avaricious of his time, to become the guide of the future observer, and also to put him in communication with Clairaut.

It is said that from his first intercourse with Lacaille, Bailly showed a decided vocation for astronomy. This fact appears to me incontestable. At his first appearance in this line, I find him associated in the most laborious, difficult and tiresome investigations of that great observer.

These epithets may perhaps appear extraordinary; but they will be so only to those who have learnt the science of the stars in ancient poems, either in verse or in prose.

The Chaldæans, luxuriously reclining on the perfumed terraced roofs of their houses in Babylon, under a constantly azure sky, followed with their eyes the general and majestic movements of the starry sphere; they ascertained the respective displacements of the planets, the moon, the sun; they noted the date and hour of eclipses; they sought out whether simple periods would not enable them to foretell these magnificent phenomena a long time beforehand. Thus the Chaldæans created, if I may be allowed the expression, *Contemplative Astronomy*. Their observations were neither numerous nor exact; they both made and discussed them without labour and without trouble.

Such is not, by a great deal, the position of modern astronomers. Science has felt the necessity of the celestial motions being studied in their minutest details. Theories must explain these details; it is their touchstone; it is by details that theories become confirmed or fall to the ground. Besides, in Astronomy, the most important truths, the most astonishing results, are based on the measurement

of quantities of extreme minuteness. Such measures, the presen bases of the science, require very fatiguing attention, infinite care to which no learned man would bind himself, were he not sustained and encouraged by the hope of attaining some capital determination through an ardent and decided devotion to the subject.

The modern astronomer, really worthy of the name, must renounce the distractions of society, and even the refreshment o uninterrupted sleep. In our climates, during the inclement season the sky is almost constantly overspread by a thick curtain o clouds. Under pain of postponing by some centuries the verification of this or that theoretic point, we must watch the leas clearing off, and avail ourselves of it without delay.

A favourable wind arises and dissipates the vapours in the very direction where some important phenomenon will manifest itself and is to last only a few seconds. The astronomer, exposed to all the transitions of weather (it is one of the conditions of accuracy), the body painfully bent, directs the telescope of a great graduated circle in haste upon the star that he impatiently awaits. His lines for measuring are a spider's threads. If in looking he makes a mistake of half the thickness of one of these threads, the observation is good for nothing; judge what his uneasiness must be: at the critical moment, a puff of wind occasioning a vibration in the artificial light adapted to his telescope, the threads become almost invisible; the star itself, whose rays reach the eye through atmospheric strata of various density, temperature, and refrangibility, will appear to oscillate so much as to render the true position of it almost unassignable; at the very moment when extremely good definition of the object becomes indispensable to insure correctness of measures, all becomes confused, either because the eye-piece gets steamed with vapour, or that the vicinity of the very cold metal occasions an abundant secretion of tears in the eye applied to the telescope: the poor observer is then exposed to the alternative of abandoning to some other more fortunate person than himself, the ascertaining a phenomenon that will not recur during his lifetime, or introducing into the science results of problematical correctness. Finally, to complete the observation, he must read off the microscopical divisions of the graduated circle, and for what opticians call *indolent vision* (the only sort that the ancients ever required) must substitute *strained vision*, which in a few years brings on blindness.*

* This long list of supposed difficulties in making an exact observation is hardly worthy of a zealous astronomer. Our author shows no enthusiasm for his subject here, and ends by ascribing the whole jeremiad to Lacaille, a man of very great practical perseverance. It is to be regretted that Arago never refers to observations of his own, but constantly quotes from others, nor does he always select the best.—*Translator's Note.*

When he has scarcely escaped from this physical and moral torture, and the astronomer wishes to know what degree of utility is deducible from his labours, he is obliged to plunge into numerical calculations of a repelling length and intricacy. Some observations that have been made in less than a minute, require a whole day's work in order to be compared with the tables.

Such was the view that Lacaille, without any softening, exhibited to his young friend; such was the profession into which the adolescent poet plunged with great ardour, and without having been at all prepared for the transition.

A useful calculation constituted the first claim of our Tyro to the attention of the learned world.

The year 1759 had been marked by one of those great events, the memory of which is religiously preserved in scientific history. A comet, that of 1682, had returned at the epoch foretold by Clairaut, and very nearly in the region that mathematical analysis had indicated to him. This re-appearance raised comets out of the category of sublunary meteors; it gave them definitely closed curves as orbits, instead of parabolas, or even mere straight lines; attraction confined them within its immense domain; in short, these bodies ceased for ever to be liable to superstition regarding them as prognostics.

The stringency, the importance of these results, would naturally increase in proportion as the resemblance between the announced orbit and the real orbit became more evident.

This was the motive that determined so many astronomers to calculate the orbit of the comet minutely, from the observations made in 1759, throughout Europe. Bailly was one of those zealous calculators. In the present day, such a labour would scarcely deserve special mention; but we must remark that the methods at the close of the eighteenth century were far from being so perfect as those that are now in use, and that they greatly depended on the personal ability of the individual who undertook them.

Bailly resided in the Louvre. Being determined to make the theory and practice of astronomy advance together, he had an observatory established from the year 1760, at one of the windows in the upper story of the south gallery. Perhaps I may occasion surprise by giving the pompous name of *Observatory* to the space occupied by a window, and the small number of instruments that it could contain. I admit this feeling, provided it be extended to the Royal Observatory of the epoch, to the old imposing and severe mass of stone that attracts the attention of the promenaders in the great walk of the Luxembourg. There also, the astronomers were obliged to stand in the hollow of the windows; there

also they said, like Bailly: I cannot verify my quadrants either by the horizon or by the zenith, for I can neither see the horizon nor the zenith. This ought to be known, even if it should disturb the wild reveries of two or three writers, who have no scientific authority: France did not possess an observatory worthy of her, nor worthy of the science, and capable of rivalling the other observatories of Europe, until within these ten or twelve years.

The earliest observations made by Bailly, from one of the windows in the upper story of the Louvre gallery that looks out on the Pont des Arts, are dated in the beginning of 1760. The pupil of Lacaille was not yet twenty-four years old. Those observations relate to an opposition of the planet Mars. In the same year he determined the oppositions of Jupiter and of Saturn, and compared the results of his own determinations with the tables.

The subsequent year I see him associated with Lacaille in observing the transit of Venus over the sun's disc. It was an extraordinary piece of good fortune, Gentlemen, at the very commencement of his scientific life, to witness in succession two of the most interesting astronomical events: the first predicted and well established return of a comet; and one of those partial eclipses of the sun by Venus, that do not recur till after the lapse of a hundred and ten years, and from which science has deduced the indirect but exact method, without which we should still be ignorant of the fact that the sun's mean distance from our earth is thirty-eight millions of leagues.

I shall have completed the enumeration of Bailly's astronomical labours performed before he became an academician, when I have added, from observations of the comet of 1762, the calculation of its parabolic orbit; the discussion of forty-two observations of the moon by La Hire, a detailed labour destined to serve as a starting-point for any person occupying himself with the lunar theory; finally, also the reduction of 515 zodiacal stars, observed by Lacaille in 1760 and 1761.

BAILLY A MEMBER OF THE ACADEMY OF SCIENCES. — HIS RESEARCHES ON JUPITER'S SATELLITES.

Bailly was named member of the Academy of Sciences the 29th January, 1763. From that moment his astronomical zeal no longer knew any bounds. The laborious life of our fellow-academician might, on occasion, be set up against a line, more fanciful than true, by which an ill-natured poet stigmatized academical honours. Certainly no one would say of Bailly, that after his election,

> "Il s'endormit et ne fit qu'un somme.
> "He fell asleep and made but one nap (or sum)."

On the contrary, we cannot but be surprised at the multituc of literary and scientific labours that he accomplished in a fe years.

Bailly's earliest researches on Jupiter's satellites began in 176: The subject was happily chosen. Studying it in all its gene ralities, he showed himself both an indefatigable computer, a clea sighted geometer, and an industrious and able observer. Bailly' researches on the satellites of Jupiter, will always be his first an chief claim to scientific glory. Before him, the Maraldis, th Bradleys, the Wargentins had discovered empirically some of th principal perturbations that those bodies undergo, in their revolvin motions around the powerful planet that rules them; but they ha not been traced up to the principles of universal attraction. Th initiative honour in this respect belongs to Bailly. Nor is thi honour decreased by the ulterior and considerable improvement that the science has since received; even the discoveries of La grange and of Laplace have left this honour intact.

The knowledge of the satellitic motions rests almost entirely o the observation of the precise moment when each of those bodie disappears, by entering into the conical shadow, which the immens opaque globe of Jupiter projects on the opposite side from the sur In the course of discussing a multitude of these eclipses, Baill was not long in perceiving that the computers of the Satelliti Tables worked on numerical data that were not at all comparabl with each other. This seemed of little consequence previous to th birth of the theory; but, after the analytical discovery of the per turbations, it became desirable to estimate the possible errors c observation, and to suggest means for remedying them. This wa the object of the very considerable work that Bailly presented t the Academy in 1771.

In this beautiful memoir, the illustrious astronomer develope the series of experiments, by the aid of which each observatio may give the instant of the real disappearance of a satellite, dis tinguished from the instant of the apparent disappearance, what ever be the power of the telescope used, whatever be the altitud of the eclipsed body above the horizon, and consequently, whateve be the transparency of the atmospheric strata through which th phenomenon is observed, also whatever be the distance from th body to the sun, or to the planet; finally, whatever be the ser sibility of the observer's sight, all which circumstances considerabl influence the time of apparent disappearance. The same series ingenious and delicate observations led the author, very curiousl to the determination of the true diameters of the satellites, that to say, of small luminous points, which, with the telescopes then use, showed no perceptible diameter.

I will rest contented with these general considerations; only remarking, in addition, that the diaphragms used by Bailly were not intended only to diminish the quantity of light contributing to the formation of the images, but that they considerably increase the diameter, and in a variable way, at least in the instance of stars.

Under this new aspect, it will be requisite to submit the question to a new examination.

Any geometers and astronomers who wish to know all the extent of Bailly's labours, must not content themselves with consulting the collections in the Academy of Sciences; for he published, at the beginning of 1766, a separate work under the modest title of *Essay on the Theory of Jupiter's Satellites.*

The author commences with the *Astronomical History of the Satellites.* This history contains an almost complete analysis of the discoveries by Maraldi, by Bradley, by Wargentin. The labours of Galileo and his contemporaries are given with less detail and exactness. I have thought that I ought to fill up the lacunæ, by availing myself of some very precious documents published a few years since, and which were unknown to Bailly.

But this I will do in a separate notice, free from all preconceived ideas, and free from all party spirit; I will not forget that an honest man ought not to calumniate any one, not even the agents of the Inquisition.

BAILLY'S LITERARY WORKS. — HIS BIOGRAPHIES OF CHARLES V. — OF LEIBNITZ — OF PETER CORNEILLE — OF MOLIÈRE.

When Bailly entered the Academy of Sciences, the perpetual secretary was Grandjean de Fouchy. The bad health of this estimable scholar occasioned an early vacancy to be foreseèn. D'Alembert cast his views on Bailly, hinted to him the survivorship to Fouchy, and proposed to him, by way of preparing the way, to write some biographies. Bailly followed the advice of the illustrious geometer, and chose as the subject of his studies, the éloges proposed by several academies, though principally by the French Academy.

From the year 1671 to the year 1758, the prize-subjects proposed by the French Academy related to questions of religion and morality. The eloquence of the candidates had therefore had to exercise itself successively on the knowledge of salvation; on the merit and dignity of martyrdom; on the purity of the soul and of the body; on the danger there is in certain paths that appear safe, &c., &c. It had even to paraphrase the *Ave Maria.* According to the literal intentions of the founder (Balzac), each discourse was ended by a short prayer. Duclos thought in 1758, that five or

six volumes of similar sermons must have exhausted the matter and on his proposal the Academy decided that, in future, it would give as the subject of the eloquence prize, the eulogiums of the great men of the nation. Marshal Saxe, Duguay Trouin, Sully, D'Aguesseau, Descartes, figured first on this list. Later, the Academy felt itself authorised to propose the éloge of kings themselves; it entered on this new branch at the beginning of 1767, by asking for the éloge of Charles V.

Bailly entered the lists, but his essay obtained only an honourable mention.

Nothing is more instructive than to search out at what epoch originated the principles and opinions of persons who have acted an important part on the political scene, and how those opinions developed themselves. By a fatality much to be regretted, the elements of these investigations are rarely numerous or faithful. We shall not have to express these regrets relative to Bailly. Each composition shows us the serene, candid, and virtuous mind of the illustrious writer, in a new and true point of view. The éloge of Charles V. was the starting-point, followed by a long series of works, and it ought to arrest our attention for a while.

The writings, crowned with the approbation of the French Academy, did not reach the public eye till they had been submitted to the severe censure of four Doctors in Theology. A special and digested approbation by the high dignitaries of the Church, whom the illustrious assembly always possessed among her members, was not a sufficient substitute for the humbling formality. If we are sure that we possess the éloge of Charles V. such as it flowed from the author's pen; if we have not reason to fear that the thoughts have undergone some mutilation, we owe it to the little favour that the discourse of Bailly enjoyed in the sitting of the Academy in 1767. Those thoughts, however, would have defied the most squeamish mind, the most shadowy susceptibility. The panegyrist unrolls with emotion the frightful misfortunes that assailed France during the reign of King John. The temerity, the improvidence of that monarch; the disgraceful passions of the King of Navarre; his treacheries; the barbarous avidity of the nobility; the seditious disposition of the people; the sanguinary depredations of the great companies; the ever recurring insolence of England; all this is expressed without disguise, yet with extreme moderation. No trait reveals, no fact even foreshadows in the author, the future President of a reforming National Assembly, still less the Mayor of Paris, during a revolutionary effervescence. The author may make Charles V. say that he will discard favour, and will call in renown to select his representatives; it will appear to him that taxes ought to be

laid on riches, and spared on poverty; he may even exclaim that oppression awakens ideas of equality. His temerity will not over-leap this boundary. Bossuet, Massillon, Bourdaloue, made the Chair resound with bold words of another description.

I am far from blaming this scrupulous reserve; when moderation is united to firmness, it becomes power. In a word, however, Bailly's patriotism might, I was about to say ought to, have shown itself more susceptible, more ardent, prouder. When in the elegant prosopopœia which closes the éloge, the King of England has recalled with arrogance the fatal day of Poitiers, ought he not instantly to have restrained that pride within just limits? ought he not to have cast a hasty glance on the components of the Black Prince's army? to examine whether a body of troops, starting from Bourdeaux, recruiting in Guienne, did not contain more Gascons than English? whether France, now bounded by its natural limits, in its magnificent unity, would not have a right, everything being examined, to consider that battle almost as an event of civil war? ought he not, in short, to have pointed out, in order to corroborate his remarks, that the knight to whom King John surrendered himself, Denys de Morbecque, was a French officer banished from Artois?

Self-reliance on the field of battle is the first requisite for obtaining success; now, would not our self-reliance be shaken, if the men most likely to know the facts and to appreciate them wisely, appeared to think that the Frank race were nationally inferior to other races who had peopled this or that region, either neighbouring or distant? This, let it be well remarked, is not a puerile susceptibility. Great events may, on a given day, depend on the opinion that the nation has formed of itself. Our neighbours on the other side of the Channel, afford examples on this subject, that it would be well to imitate.

In 1767, the Academy of Berlin proposed a prize for an éloge of Leibnitz. The public was somewhat surprised at it. It was generally supposed that Leibnitz had been admirably praised by Fontenelle, and that the subject was exhausted. But from the moment that Bailly's essay, crowned in Prussia, was published, former impressions were quite changed. Every one was anxiously asserting that Bailly's appreciation of his subject might be read with pleasure and benefit, even after Fontenelle's. The éloge composed by the historian of Astronomy will not, certainly, make us forget that written by the first Secretary of the Academy of Sciences. The style is, perhaps, too stiff; perhaps it is also rather declamatory; but the biography, and the analysis of his works, are more complete, especially if we consider the notes;

the *universal* Leibnitz is exhibited under more varied points o
view.

In 1768, Bailly obtained the award of the prize of eloquenc
proposed by the Academy of Rouen. The subject was th
éloge of Peter Corneille. In reading this work of our fellow
academician, we may be somewhat surprised at the immense dis
tance that the modest, the timid, the sensitive Bailly puts betwee
the great Corneille, his special favourite, and Racine.

When the French Academy, in 1768, proposed an éloge o
Molière for competition, our candidate was vanquished only b
Chamfort. And yet, if people had not since that time treated o
the author of "Tartufe" to satiety, perhaps I would venture t
maintain, notwithstanding some inferiority of style, that Bailly
discourse offered a neater, truer, and more philosophic appreciatio
of the principal pieces of that immortal poet.

DEBATES RELATIVE TO THE POST OF PERPETUAL SECRETARY OF TH ACADEMY OF SCIENCES.

We have seen D'Alembert, ever since the year 1763, encou
raging Bailly to exercise himself in a style of literary compositio
then much liked, the style of éloge, and holding out to him i
prospect the situation of Perpetual Secretary of the Academ
of Sciences. Six years after, the illustrious geometer gave th
same advice, and perhaps held out the same hopes, to the youn
Marquis de Condorcet. This candidate, docile to the voice of hi
protector, rapidly composed and published the éloges of th
early founders of the Academy, of Huyghens, of Mariotte, o
Roëmer, &c.

At the beginning of 1773, the Perpetual Secretary, Grandjea
de Fouchy, requested that Condorcet should be nominated his suc
cessor provided he survived him. D'Alembert strongly supporte
this candidateship. Buffon supported Bailly with equal energy
the Academy presented for some weeks the aspect of two hostil
camps. There was at last a strongly disputed electoral battle
the result was the nomination of Condorcet.

I should regret if we had to judge of the sentiments of Baill
after this defeat, by those of his adherents. Their anger found ve
in terms of unpardonable asperity. They said that D'Alembe
had "basely betrayed friendship, honour, and the first principles o
probity."

They here alluded to a promise of protection, support, co-opera
tion, dating ten years back. But was his promise absolute
Engaging himself personally to Bailly for a situation that migh
not become vacant for ten or fifteen years, had d'Alembert, con
trary to his duty as an academician, declared beforehand, that an

other candidate, whatever might be his talents, would be to him as not existing?

This is what ought to have been ascertained, before giving themselves up to such violent and odious imputations.

Was it not quite natural that the geometer D'Alembert, having to pronounce his opinion between two honourable learned men, gave the preference to the candidate who seemed to him most imbued with the higher mathematics? The éloges of Condorcet were, besides, by their style, much more in harmony with those that the Academy had approved during three-quarters of a century. Before the declaration of the vacancy on the 27th Feb., 1773, D'Alembert said to Voltaire relative to the recueil by Condorcet, "Some one asked me the other day what I thought of that work. I answered by writing on the frontispiece 'Justice, propriety, learning, clearness, precision, taste, elegance, and nobleness.'" And Voltaire wrote on the 1st of March, "I have read, while dying, the little book by M. de Condorcet; it is as good in its departments as the éloges by Fontenelle. There is a more noble and more modest philosophy in it, though bold."

And excitement in words and action could not be legitimately reproached in a man who had felt himself supported by a conviction of such distinct and powerful influence.

Among the éloges by Bailly, there is one, that of the Abbé de Lacaille, which not having been written for a literary academy, shows no longer any trace of inflation or declamation, and might, it seems to me, compete with some of the best éloges by Condorcet. Yet, it is curious, that this excellent biography contributed, perhaps as much as D'Alembert's opposition, to make Bailly's claims fail. Vainly did the celebrated astronomer flatter himself in his exordium, "that M. de Fouchy, who, as Secretary of the Academy, had already paid his tribute to Lacaille, would not be displeased at his having followed him in the same career . . . . . that he would not be blamed for repeating the praises due to an illustrious man."

Bailly, in fact, was not blamed aloud; but when the hour for retreat had sounded in M. de Fouchy's ear, without any fuss, without showing himself offended in his self-love, remaining apparently modest, this learned man, in asking for an assistant, selected one who had not undertaken to repeat his éloges; who had not found his biographies insufficient. This preference ought not to be, and was not, uninfluential in the result of the competition.

Bailly, if Perpetual Secretary of the Academy, would have been obliged to reside constantly at Paris. But Bailly, as member of the Astronomical Section, might retire to the country, and thus

escape those thieves of time, as Byron called them, who especially abound in the metropolis. Bailly settled at Chaillot. It was at Chaillot that our fellow-academician composed his best works, those that will sail down the stream of time.

Nature had endowed Bailly with the most happy memory. He did not write his discourses till he had completed them in his head. His first copy was always a clean copy. Every morning, Bailly started early from his humble residence at Chaillot; he went to the Bois de Boulogne, and there, walking for many hours at a time, his powerful mind elaborated, co-ordinated, and robed in all the pomps of language, those high conceptions destined to charm successive generations. Biographers inform us, that Crébillon composed in a similar way. And this was, according to several critics, the cause of the incorrectness, of the asperity of style, which disfigure several pieces by that tragic poet. The works of Bailly, and especially the discourses that complete the *History of Astronomy*, invalidate this explanation. I could also appeal to the elegant and pure productions of that poet, whom France has just lost and weeps for. No one indeed can be ignorant of his works; Casimir Delavigne, like Bailly, never committed his verses to paper until he had worked them up in his mind to that harmonious perfection which procured for them the unanimous suffrages of all people of taste. Gentlemen, pardon this reminiscence. The heart loves to connect such names as those of Bailly and of Delavigne; those rare and glorious symbols, in whom we find united talent, virtue, and an invariable patriotism.

HISTORY OF ASTRONOMY. — LETTERS ON THE ATLANTIS OF PLATO AND ON THE ANCIENT HISTORY OF ASIA.

In 1775, Bailly published a quarto volume, entitled *History of Ancient Astronomy, from its Origin up to the Establishment of the Alexandrian School.* An analogous work for the lapse of time, comprised between the Alexandrian School and 1730, appeared in 1779, in two volumes. An additional volume appeared three years later, entitled the *History of Modern Astronomy up to the Epoch of* 1782. The fifth part of this immense composition, the *History of Indian Astronomy,* was published in 1787.

When Bailly undertook this general history of Astronomy, the science possessed nothing of the sort. Erudition had seized upon some special questions, some detailed points, but no commanding view had presided over these investigations.

Weidler's book, published in 1741, was a mere simple nomenclature of the astronomers of every age, and of every country; the dates of their birth and death; the titles of their works. The

utility of this precise enumeration of dates and titles did not alter the character of the book.

Bailly sketches the plan of his work with a masterly hand in a few lines; he says, "It is interesting to transport oneself back to the times when Astronomy began; to observe how discoveries were connected together, how errors have got mixed up with truth, have delayed the knowledge of it, and retarded its progress; and, after having followed the various epochs and traversed every climate, finally to contemplate the edifice founded on the labours of successive centuries and of various nations."

This vast plan essentially led to the minute discussion and comparison of a multitude of passages both ancient and modern. If the author had mixed up these discussions with the body of the work, he would have laboured for astronomers only. If he had suppressed all discussions, the book would have interested amateurs only. To avoid this double rock, Bailly decided on writing a connected narrative with the quintessence of the facts, and to place the proofs and the discussions of the merely conjectural parts, under the appellation of explanations in separate chapters. Bailly's History, without forfeiting the character of a serious and erudite work, became accessible to the public in general, and contributed to disseminate accurate notions of Astronomy both among literary men and among general society.

When Bailly declared, in the beginning of his book, that he would go back to the very commencement of Astronomy, the reader might expect some pages of pure imagination. I know not, however, whether any body would have expected a chapter of the first volume to be entitled, *Of Antediluvian Astronomy.*

The principal conclusion to which Bailly comes, after an attentive examination of all the positive ideas that antiquity has bequeathed to us is, that we find rather the ruins than the elements of a science in the most ancient Astronomy of Chaldæa, of India, and of China.

After treating of certain ideas of Pluche, Bailly says, "The country of possibilities is immense, and although truth is contained therein, it is not often easy to distinguish it."

Words so reasonable would authorise me to inquire whether the calculations of our fellow-labourer, intended to establish the immense antiquity of the Indian Tables, are beyond all criticism. But the question has been sufficiently discussed in a passage of *The Exposition of the System of the World,* on which it would be useless to insist here. Whatever came from the pen of M. de Laplace was always marked by the stamp of reason and of evidence. In the first lines of his magnificent work, after having remarked that "the history of Astronomy forms an essential part of the his-

tory of the human mind," Bailly observes, "that it is perhaps the true measure of man's intelligence, and a proof of what he can do with time and genius." I shall allow myself to add, that no study offers to reflecting minds more striking or more curious relations.

When by measurements, in which the evidence of the method advances equally with the precision of the results, the volume of the earth is reduced to the millionth part of the volume of the sun; when the sun himself, transported to the region of the stars, takes up a very modest place among the thousands of millions of those bodies that the telescope has revealed to us; when the 38,000,000 of leagues which separate the earth from the sun, have become, by reason of their comparative smallness, a base totally insufficient for ascertaining the dimensions of the visible universe; when even the swiftness of the luminous rays (77,000 leagues per second) barely suffices for the common valuations of science; when, in short, by a chain of irresistible proofs, certain stars have retired to distances that light could not traverse in less than a million of years; we feel as if annihilated by such immensities. In assigning to man, and to the planet that he inhabits, so small a position in the material world, Astronomy seems really to have made progress only to humble us.

But if, on the other hand, we regard the subject from the opposite point of view, and reflect on the extreme feebleness of the natural means by the help of which so many great problems have been attacked and solved; if we consider that to obtain and measure the greater part of the quantities now forming the basis of astronomical computation, man has had greatly to improve the most delicate of his organs, to add immensely to the power of his eye; if we remark that it was not less requisite for him to discover methods adapted to measuring very long intervals of time, up to the precision of tenths of seconds; to combat against the most microscopic effects that constant variations of temperature produce in metals, and therefore in all instruments; to guard against the innumerable illusions that a cold or hot atmosphere, dry or humid, tranquil or agitated, impresses on the medium through which the observations have inevitably to be made; the feeble being resumes all his advantage: by the side of such wonderful labours of the mind, what signifies the weakness, the fragility of our body; what signify the dimensions of the planet, our residence, the grain of sand on which it has happened to us to appear for a few moments!

The thousands of questions on which Astronomy has thrown its dazzling light belong to two entirely distinct categories; some offered themselves naturally to the mind, and man had only to seek the means for solving them; others, according to the beautiful

expression of Pliny, were enveloped in the majesty of nature! When Bailly lays down in his book these two kinds of problems, it is with the firmness, the depth, of a consummate astronomer; and when he shows their importance, their immensity, it is always with the talent of a writer of the highest order; it is sometimes with a bewitching eloquence. If in the beautiful work we are alluding to, Astronomy unavoidably assigns to man an imperceptible place in the material world, she assigns him, on the other hand, a vast share in the intellectual world. The writings which, supported by the invincible deductions of science, thus elevate man in his own eyes, will find grateful readers in all climes and times.

In 1775, Bailly sent the first volume of his history to Voltaire. In thanking him for his present, the illustrious old man addressed to the author one of those letters that he alone could write, in which flattering and enlivening sentences were combined without effort with high reasoning powers. "I have many thanks to return you (said the Patriarch of Ferney), for having on the same day received a large book on medicine and yours, while I was still ill; I have not opened the first, I have already read the second almost entirely, and feel better."

Voltaire, indeed, had read Bailly's work pen in hand, and he proposed to the illustrious astronomer some queries, which proved both his infinite perspicacity, and wonderful variety of knowledge. Bailly then felt the necessity of developing some ideas which in his *History of Ancient Astronomy* were only accessories to his principal subject. This was the object of the volume that he published in 1776, under the title of *Letters on the Origin of the Sciences and of the People of Asia, addressed to M. de Voltaire.* The author modestly announced that "to lead the reader by the interest of the style to the interest of the question discussed," he would place at the head of his work three letters from the author of *Merope*, and he protested against the idea that he had been induced to play with paradoxes.

According to Bailly, the present nations of Asia are heirs of an anterior people, who understood Astronomy perfectly. Those Chinese, those Hindoos, so renowned for their learning, would thus have been mere depositaries; we should have to deprive them of the title of inventors. Certain astronomical facts, found in the annals of those southern nations, appear to have belonged to a higher latitude. By these means we discover the true site on the globe of the primitive people, proving against the received opinion that learning came southward from the north.

Bailly also found that the ancient fables, considered physically appeared to belong to the northern regions of the earth.

In 1779, Bailly published a second collection, forming a seque

to the former, and entitled *Letters on the Atlantis of Plato, and on the Ancient History of Asia.*

Voltaire died before these new letters could be communicated to him. Bailly did not think that this circumstance ought to make him change the form of the discussion already employed in the former series; it is still Voltaire whom he addresses.

The philosopher of Ferney thought it strange that there should be no knowledge of this ancient people, who, according to Bailly, had instructed the Indians. To answer this difficulty, the celebrated astronomer undertakes to prove that some nations have disappeared, without their existence being known to us by anything beyond tradition. He cites five of these, and in the first rank the Atlantidæ.

Aristotle said that he thought Atlantis was a fiction of Plato's: "He who created it also destroyed it, like the walls that Homer built on the shores of Troy, and then made them disappear." Bailly does not join in this scepticism. According to him, Plato spoke seriously to the Athenians of a learned, polished people, but destroyed and forgotten. Only, he totally repudiates the idea of the Canaries being the remains of the ancient country of the Atlantidæ, and now engulfed. Bailly rather places that nation at Spitzbergen, Greenland, or Nova Zembla, whose climate may have changed. We should also have to seek for the Garden of the Hesperides near the Pole; in short, the fable of the Phœnix may have arisen in the Gulf of the Obi, in a region where we must suppose the sun to have been annually absent during sixty-five days.

It is evident, in many passages, that Bailly is himself surprised at the singularity of his own conclusions, and fears that his readers may rather regard them as jokes. He therefore exclaims, "My pen would not find expressions for thoughts which I did not believe to be true." Let us add, that no effort is painful to him. Bailly calls successively to his aid astronomy, history, supported by vast erudition, philology, the systems of Mairan, of Buffon, relatively to the heat appertaining to the earth. He does not forget, using his own words, "that in the human species, still more sensitive than curious, more anxious for pleasure than for instruction, nothing pleases generally, or for a long time, unless the style is agreeable; that dry truth is killed by ennui!" Yet Bailly makes few proselytes; and a species of instinct determines men of science to despise the fruits of so persevering a labour; and D'Alembert goes so far as to tax them with poverty, even with hollow ideas, with vain and ridiculous efforts; he goes so far as to call Bailly, relatively to his letters, the *illuminated brother.* Voltaire is, on the contrary, very polite and very academical in his communications with our author. The renown of the Brahmins is

dear to him; yet this does not prevent his discussing closely the proofs, the arguments of the ingenious astronomer. We could also now enter into a serious discussion. The mysterious veil that in Bailly's time covered the East, is in great part raised. We now know the Astronomy of the Chinese and the Hindoos in all its detail. We know up to what point the latter had carried their mathematical knowledge. The theory of central heat has in a few years made an unhoped-for progress; in short, comparative philology, prodigiously extended by the invaluable labours of Sacy, Rémusat, Quatremère, Burnouf, and Stanislaus Julien, have thrown strong lights on some historical and geographical questions, where there reigned before a profound darkness. Armed with all these new means of investigation, it might easily be established that the systems relative to an ancient unknown people, first creator of all the sciences, and relative to the Atlantidæ, rest on foundations devoid of solidity. Yet, if Bailly still lived, we should be only just in saying to him, as Voltaire did, merely changing the tense of a verb, "Your two books *were*, Sir, treasures of the most profound erudition and the most ingenious conjectures, adorned with an eloquence of style, which is always suitable to the subject."

FIRST INTERVIEW OF BAILLY WITH FRANKLIN. — HIS ENTRANCE INTO THE FRENCH ACADEMY IN 1783. — HIS RECEPTION. — DISCOURSE. — HIS RUPTURE WITH BUFFON.

Bailly became the particular and intimate friend of Franklin at the end of 1777. The personal acquaintance of these two distinguished men began in the strangest manner.

One of the most illustrious members of the Institute, Volney, on returning from the New World, said: "The Anglo-Americans tax the French with lightness, with indiscretion, with chattering." (Volney, preface to *The Table of the Climate of the United States.*) Such is the impression, in my opinion very erroneous, at least by comparison, under which the Ambassador Franklin arrived in France. All the world knows that he halted at Chaillot. As an inhabitant of the Commune, Bailly thought it his duty to visit without delay the illustrious guest thus received. He was announced, and Franklin, knowing him by reputation, welcomed him very cordially, and exchanged with his visitor the eight or ten words usual on such occasions. Bailly seated himself by the American philosopher, and discreetly awaited some question to be put to him. Half an hour passed, and Franklin had not opened his mouth. Bailly drew out his snuff-box, and presented it to his neighbour without a word; the traveller signed with his hand that he did not take snuff. The dumb interview was then prolonged during a whole hour. Bailly finally rose. Then Franklin

as if delighted to have found a Frenchman who could remain silen extended his hand to him, pressed his visitor's affectionately, ex claiming: "Very well, Monsr. Bailly, very well!"

After having recounted the anecdote as our academician use amusingly to relate it, I really fear being asked how I look upo it. Well, Gentlemen, whenever this question may be put to me I shall answer that Bailly and Franklin discussing together som scientific question from the moment of their meeting, would hav appeared to me much more worthy of each other, than the tw actors of the scene at Chaillot. I will, moreover, grant that w may draw the following inference,—that even men of genius ar liable to cross humours; but I must at the same time add tha the example is not dangerous, dumbness not being an efficaciou method of making oneself valued, or of distinguishing ourselves t advantage.

Bailly was nominated member of the French Academy in the place of M. de Tressan, in Nov., 1783. The same day, M. de Choiseul Gouffier succeeded to D'Alembert. Thanks to the coincidence of the two nominations, Bailly escaped the sarcasms which the expectant academicians never fail to pour out, with or without reason, against those who have obtained a double crown. This time they vented their spleen exclusively on the great man, thus enabling the astronomer to take possession of his new dignity without raising the usual storm. Let us carefully collect, Gentlemen, from the early years of our academician's life, all that may appear an anticipated compensation for the cruel trials that we shall have to relate in the sequel.

The admission of the eloquent author of the *History of Astronomy* into the Academy, was more difficult than could be supposed by those who have remarked to what slight works certain early and recent writers have owed the same favour. Bailly failed three times. Fontenelle had before him unsuccessfully presented himself once oftener; but Fontenelle underwent these successive checks without ill-humour, and without being discouraged. Bailly, on the contrary, with or without reason, seeing in these unfavourable results of the elections the immediate effect of D'Alembert's enmity, showed himself much more hurt at it, perhaps, than was suitable for a philosopher. In these somewhat envenomed contests, Buffon always gave Bailly a cordial and able support.

Bailly pronounced his reception-discourse in February, 1784. The merits of M. de Tressan were therein celebrated with grace and delicacy. The panegyrist identified himself with his subject. A select public loaded with praises various passages wherein just and profound ideas were clothed in all the richness of a forcible and harmonious style.

Did any one ever speak with more eloquence of the scientific

power revealed by a contemporary discovery! Listen, Gentlemen, and judge.

"That which the sciences can add to the privileges of the human race has never been more marked than at the present moment. They have acquired new domains for man. The air seems to become as accessible to him as the waters, and the boldness of his enterprises equals almost the boldness of his thoughts. The name of Montgolfier, the names of those hardy navigators of the new element, will live through time; but who among us, on seeing these superb experiments, has not felt his soul elevated, his ideas expanded, his mind enlarged?"

I know not whether, all things considered, the satisfaction of self-love which may be attached to academical titles, to his success in public and important meetings, ever completely rewarded Bailly for the heart-aches he experienced in his literary career.

A kind and tender intimacy had grown up between the great naturalist Buffon and the celebrated astronomer. An academical nomination broke it up. You know it, Gentlemen; amongst us a nomination is the apple of discord: notwithstanding the most opposite views, every one then thinks that he is acting for the true interest of science or of letters; every one thinks that he is proceeding in the line of strict justice; every one endeavours earnestly to make proselytes. So far all is legitimate. But what is much less so, is forgetting that a vote is a decision, and that in this sense the academician, like the magistrate, may say to the suitor, whether an academician or not, "I give decrees, and not services."

Unfortunately, considerations of this sort, notwithstanding their justice, would make but little impression on the haughty and positive mind of Buffon. That great naturalist wished to have the Abbé Maury nominated; his associate Bailly thought he ought to vote for Sedaine. Let us place ourselves in the ordinary course of things, and it will appear difficult to see in this discordancy a sufficient cause for a rupture between two superior men. *The Unforeseen Wager* and *The Unconscious Philosopher*, considerably balanced the, then very light, weight of Maury. The comic poet had already reached his sixty-sixth year; the Abbé was young. The high character, the irreproachable conduct of Sedaine, might, without disparagement, be put in comparison with what the public knew of the character of the official and the private life of the future cardinal. Whence then had the illustrious naturalist derived such a great affection for Maury, such violent antipathies against Sedaine? It may be surmised that they arose from aristocratic prejudices of rank. Nor is it impossible but that M. le Comte de Buffon instinctively foresaw, with some repugnance, his approaching confraternity with a man formerly a lapidary; but was not Maury

the son of a shoemaker? This very small incident of our literary history seemed doomed to remain in obscurity; chance has, I believe, given me the key to it.

You remember, Gentlemen, that aphorism continually quoted by Buffon, and to which he seemed very proud, —

> "Style makes the man."

I have discovered that Sedaine made a counterpart of it. The author of *Richard Cœur de Lion* and of *The Deserter* said, —

> "Style is nothing, or next to it!"

Place this heresy, in imagination, under the eyes of the immortal writer, whose days and nights were passed in polishing his style, and if you then ask me why he detested Sedaine, I shall have a right to answer: You do not know the human heart.

Bailly firmly resisted the imperious solicitations of his former patron, and refused even to absent himself from the Academy on the day of the nomination. He did not hesitate to sacrifice the attractions and advantages of an illustrious friendship to the performance of a duty; he answered to him who wanted to be master, "I will be free." Honour be to him!

The example of Bailly warns timid men never to listen to mere entreaties, whatever may be their source; not to yield but to good arguments. Those who have thought so little of their own tranquillity as to do any more in academical elections than to give a silent and secret vote, will see on their part, in the noble and painful resistance of an honest man, how culpable they become in trying to substitute authority for persuasion, in wishing to subject conscience to gratitude.

On the occurrence of a similar discord, the astronomer Lemonnier, of the Academy of Sciences, said one day to Lalande, his fellow-academician and former pupil, "I enjoin you not to put your foot again within my door during the semi-revolution of the lunar orbital nodes." Calculation shows this to be nine years. Lalande submitted to the punishment with a truly astronomical punctuality; but the public, despite the scientific form of the sentence, thought it excessively severe. What then will be said of that which was pronounced by Buffon?—"We will never see each other more, Sir!" These words will appear at once both harsh and solemn, for they were occasioned by a difference of opinion on the comparative merits of Sedaine and the Abbé Maury. Our friend resigned himself to this separation, nor ever allowed his just resentment to be perceived. I may even remark, that after this brutal disruption he showed himself more attentive than ever to seize opportunities

of paying a legitimate homage to the talents and eloquence of the French Pliny.

REPORT ON ANIMAL MAGNETISM.

We are now going to see the astronomer, the savant, the man of letters, struggling against passions of every kind, excited by the famous question of animal magnetism.

At the beginning of the year 1778, a German doctor established himself at Paris. This physician could not fail of succeeding in what was then styled high society. He was a stranger. His government had expelled him; acts of the greatest effrontery and unexampled charlatanism were imputed to him.

His success, however, exceeded all expectations. The Gluckists and the Piccinists themselves forgot their differences to occupy themselves exclusively with the new comer.

Mesmer, since we must call him by his name, pretended to have discovered an agent till then totally unknown both in the arts and in physics; an universally distributed fluid, and serving thus as a means of communication and of influence among the celestial globes; — a fluid capable of flux and reflux, which introduced itself more or less abundantly into the substance of the nerves, and acted on them in a useful manner, — thence the name of animal magnetism given to this fluid.

Mesmer said, "Animal magnetism may be accumulated, concentrated, transported, without the aid of any intermediate body. It is reflected like light; musical sounds propagate and augment it."

Properties so distinct, so precise, seemed as if they must be capable of experimental verification. It was requisite then to be prepared for some instance of want of success, and Mesmer took good care not to neglect it; the following was his declaration: "Although the fluid be universal, all animated bodies do not equally assimilate it into themselves; there are some even, though very few in number, that by their very presence destroy the effects of this fluid in the surrounding bodies."

So soon as this was admitted, as soon it was allowed to explain instances of non-success by the presence of neutralising bodies, Mesmer no longer ran any risk of being embarrassed. Nothing prevented his announcing, in full security, "that animal magnetism could immediately cure diseases of the nerves, and mediately other diseases; that it afforded to doctors the means of judging with certainty of the origin, the nature, and the progress of the most complicated maladies; that nature, in short, offered in magnetism a universal means of curing and preserving mankind."

Before quitting Vienna, Mesmer had communicated his systematic notions to the principal learned societies of Europe. The Aca-

demy of Sciences of Paris, and the Royal Society of London, did not think proper to answer. The Academy of Berlin examined the work, and wrote to Mesmer that he was in error.

Some time after his arrival in Paris, Mesmer tried again to get into communication with the Academy of Sciences. This society even acceded to a rendezvous. But, instead of the empty words that were offered them, the academicians required experiments. Mesmer stated—I quote his words—that *it was child's play;* and the conference had no other result.

The Royal Society of Medicine, being called upon to judge of the pretended cures performed by the Austrian doctor, thought that their agents could not give a well-founded opinion "without having first duly examined the patients to ascertain their state." Mesmer rejected this natural and reasonable proposal. He wished that the agents should be content with the word of honour and attestations of the patients. In this respect, also, the severe letters of the worthy Vicq-d'Azyr put an end to communications which must have ended unsatisfactorily.

The faculty of medicine showed, we think, less wisdom. It refused to examine any thing; it even proceeded in legal form against one of its regent doctors who had associated himself, they said, with the charlatanism of Mesmer.

These barren debates evidently proved that Mesmer himself was not thoroughly sure of his theory, nor of the efficacy of the means of cure that he employed. Still the public showed itself blind. The infatuation became extreme. French society appeared at one moment divided into magnetisers and magnetised. From one end of the kingdom to the other agents of Mesmer were seen, who, with receipt in hand, put the weak in intellect under contribution.

The magnetisers had had the address to intimate that the mesmeric crises manifested themselves only in persons endowed with a certain sensitiveness. From that moment, in order not to be ranged among the insensible, both men and women, when near the *rod,* assumed the appearance of epileptics.

Was not Father Hervier really in one of those paroxysms of the disease when he wrote, "If Mesmer had lived contemporary with Descartes and Newton, he would have saved them much labour: those great men suspected the existence of the universal fluid; Mesmer has discovered the laws of its action"?

Count de Gébelin showed himself stranger still. The new doctrine would naturally seduce him by its connection with some of the mysterious practices of ancient times; but the author of *The Primitive World* did not content himself with writing in favour of Mesmerism with the enthusiasm of an apostle. Frightful pain.

violent griefs, rendered life insupportable to him; Gébelin saw death approaching with satisfaction, so from that moment he begged earnestly that he might not be carried to Mesmer's, where assuredly "he could not die." We must just mention, however, that his request was not attended to; he was carried to Mesmer's, and died while he was being magnetised.

Painting, sculpture, and engraving were constantly repeating the features of this Thaumaturgus. Poets wrote verses to be inscribed on the pedestals of the busts, or below the portraits. Those by Palisot deserve to be quoted, as one of the most curious examples of poetic licences: —

> "Behold that man — the glory of his age!
> Whose art can all Pandora's ills assuage.
> In skill and tact no rival pow'r is known —
> E'en Greece, in him, would Esculapius own."*

Enthusiasm having thus gone to the last limits in verse, enthusiasm had but one way left to become remarkable in prose: that is, violence. Is it not thus that we must characterise the words of Bergasse? — "The adversaries of animal magnetism are men who must one day be doomed to the execration of all time, and to the punishment of the avenging contempt of posterity."

It is rare for violent words not to be followed by violent acts. Here everything proceeded according to the natural course of human events. We know, indeed, that some furious admirers of Mesmer attempted to suffocate Berthollet in the corner of one of the rooms of the Palais Royal, for having honestly said that the scenes he had witnessed did not appear to him demonstrative. We have this anecdote from Berthollet himself.

The pretensions of the German doctor increased with the number of his adherents. To induce him to permit only three learned men to attend his meetings, M. de Maurepas offered him, in the name of the king, 20,000 francs a year for life, and 10,000 annually for house-rent. Yet Mesmer did not accept this offer, but demanded, as a national recompense, one of the most beautiful châteaux in the environs of Paris, together with all its territorial dependencies.

Irritated at finding his claims repulsed, Mesmer quitted France, angrily vowing her to the deluge of maladies from which it would have been in his power to save her. In a letter written to Marie

* "Le voilà, ce mortel, dont le siècle s'honore,
Par qui sont replongés au séjour infernal
Tous les fléaux vengeurs que déchaîna Pandore;
Dans son art bienfaisant il n'a pas de rival,
Et la Grèce l'eut pris pour le dieu d'Epidaure."

Antoinette, the Thaumaturgus declared that he had refused the government offers through austerity.

Through austerity!!! Are we then to believe that, as it was then pretended, Mesmer was entirely ignorant of the French language; that in this respect his meditations had been exclusively centered on the celebrated verse —

"Fools are here below for our amusement?"*

However this may be, the austerity of Mesmer did not prevent his being most violently angry when he learnt at Spa that Deslon continued the magnetical treatments at Paris. He returned in all haste. His partisans received him with enthusiasm, and set on foot a subscription of 100 louis per head, which produced immediately near 400,000 francs (16,000*l.*). We now feel some surprise to see among the names of the subscribers, those of Messrs. de Lafayette, de Ségur, d'Eprémesnil.

Mesmer quitted France a second time about the end of 1781, in quest of a more enlightened government who could appreciate superior minds. He left behind him a great number of tenacious and ardent adepts, whose importunate conduct at last determined the government to submit the pretended magnetic discoveries to be examined by four Doctors of the Faculty of Paris. These distinguished physicians solicited to have added to them some members of the Academy of Sciences. M. de Breteuil then recommended Messrs. Le Roy, Bory, Lavoisier, Franklin, and Bailly, to form part of the mixed commission. Bailly was finally named reporter.

The work of our brother-academician appeared in August, 1784. Never was a complex question reduced to its characteristic traits with more penetration and tact; never did more moderation preside at an examination, though personal passions seemed to render it impossible; never was a scientific subject treated in a more dignified and lucid style.

Nothing equals the credulity of men in whatever touches their health. This aphorism is an eternal truth. It explains how a portion of the public has returned to mesmeric practices; how I shall still perform an interesting task by giving a detailed analysis of the magnificent labours published by our fellow-academician sixty years ago. This analysis will show besides, how daring those men were, who recently, in the bosom of another academy, constituted themselves passionate defenders of some old women's tales, which one would have supposed had been permanently buried in oblivion.

The commissioners go in the first place to the treatment by

* "Les sots sont ici-bas pour nos menus plaisirs."

M. Deslon, examine the famous rod, describe it carefully, relate the means adopted to excite and direct magnetism. Bailly then draws out a varied and truly extraordinary table of the state of the sick people. His attention is principally attracted by the convulsions that they designated by the name of *crisis*. He remarked that in the number of persons in the crisis state, there were always a great many women, and very few men; he does not imagine any deceit, however; holds the phenomena as established, and passes on to search out their causes.

According to Mesmer and his partisans, the cause of the crisis and of the less characteristic effects, resided in a particular fluid. It was to search out proofs of the existence of this fluid, that the commissioners had first to devote their efforts. Indeed, Bailly said, "Animal magnetism may exist without being useful, but it cannot be useful if it does not exist."

The animal magnetic fluid is not luminous and visible like electricity; it does not produce marked and manifest effects on inert matter, as the fluid of the ordinary magnet does; finally, it has no taste. Some magnetisers asserted that it had a smell; but repeated experiments proved that they were in error. The existence then of the pretended fluid could be established only by its effects on animated beings.

Curative effects would have thrown the commission into an inextricable dædalus, because nature alone, without any treatment, cures many maladies. In this system of observations, they could not have hoped to learn the exact part performed by magnetism, until after a great number of cures, and after trials oftentimes repeated.

The commissioners, therefore, had to limit themselves to instantaneous effects of the fluid on the animal organism.

They then submitted themselves to the experiments, but using an important precaution. "There is no individual," says Bailly, "in the best state of health, who, if he closely attended to himself, would not feel within him an infinity of movements and variations, either of exceedingly slight pain, or of heat, in the various parts of his body. . . . These variations which are continually taking place, are independent of magnetism. . . . The first care required of the commissioners was, not to be too attentive to what was passing within them. If magnetism is a real and powerful cause, we have no need to think about it to make it act and manifest itself; it must, so to say, force the attention, and make itself perceived by even a purposely distracted mind."

The commissioners, magnetised by Deslon, felt no effect. After the healthy people, some ailing ones followed, taken of all ages and from various classes of society. Among these sick people

who amounted to fourteen, five felt some effects. On the remaining nine, magnetism had no effect whatever.

Notwithstanding the pompous announcements, magnetism already could no longer be considered as a certain indicator of diseases.

Here the reporter made a capital remark: magnetism appeared to have no effect on incredulous persons who had submitted to the trials, nor on children. Was it not allowable to think, that the effects obtained in the others proceeded from a previous persuasion as to the efficacy of the means, and that they might be attributed to the influence of imagination? Thence arose another system of experiments. It was desirable to confirm or to destroy this suspicion; "it became therefore requisite to ascertain to what degree imagination influences our sensations, and to establish whether it could have been in part or entirely the cause of the effects attributed to magnetism."

There could be nothing neater or more demonstrative than this portion of the work of the commissioners. They go first to Dr. Jumelin, who, let it be observed, obtains the same effects, the same crises as Deslon and Mesmer, by magnetising according to an entirely different method, and not restricting himself to any distinction of poles; they select persons who seem to feel the magnetic action most forcibly, and put their imagination at fault by now and then bandaging their eyes.

What happens then?

When the patients see, the seat of the sensations is exactly the part that is magnetised; when their eyes are bandaged, they locate these same sensations by chance, sometimes in parts very far away from those to which the magnetiser is directing his attention. The patient whose eyes are covered often feels marked effects at a time when they are not magnetising him, and remains, on the contrary, quite passive while they are magnetising him without his being aware of it.

Persons of all classes offer similar anomalies. An instructed physician, subjected to these experiments, "feels effects whilst nothing is being done, and often does not feel effects whilst he is being acted upon. On one occasion, thinking that they had been magnetising him for ten minutes, this same doctor fancied that he felt a heat in his lumbi, which he compared to that of a stove."

Sensations thus felt, when no magnetising was exerted, must evidently have been the effect of imagination.

The commissioners were too strict logicians to confine themselves with these experiments. They had established that imagination in some individuals can occasion pain, and heat, even a considerable degree of heat, in all parts of the body; but practical

female Mesmerisers did more; they agitated certain people to that pitch, that they fell into convulsions. Could the effect of imagination go so far?

Some new experiments entirely did away with these doubts.

A young man was taken to Franklin's garden at Passy, and when it was announced to him that Deslon, who had taken him there, had magnetised a tree, this young man ran about the garden, and fell down in convulsions, but it was not under the magnetised tree: the crisis seized him while he was embracing another tree, very far from the former.

Deslon selected, in the treatment of poor people, two women who had rendered themselves remarkable by their sensitiveness around the famous rod, and took them to Passy. These women fell into convulsions whenever they thought themselves mesmerised, although they were not. At Lavoisier's, the celebrated experiment of the cup gave analogous results. Some plain water engendered convulsions occasionally, when magnetised water did not.

We must really renounce the use of our reason not to perceive a proof in this collection of experiments, so well arranged, that imagination alone can produce all the phenomena observed around the mesmeric rod, and that mesmeric proceedings, cleared from the delusions of imagination, are absolutely without effect. The commissioners, however, recommence the examination on these last grounds, multiply the trials, adopt all possible precautions, and give to their conclusions the evidence of mathematical demonstrations. They establish, finally and experimentally, that the action of the imagination can both occasion the crises to cease, and can engender their occurrence.

Foreseeing that people with an inert or idle mind, would be astonished at the important part assigned to the imagination by the commissioners' experiments in the production of mesmeric phenomena, Bailly instanced: sudden affection disturbing the digestive organs; grief giving the jaundice; the fear of fire restoring the use of their legs to paralytic patients; earnest attention stoping the hiccough; fright blanching people's hair in an instant, &c.

The touching or stroking practised in mesmeric treatments, as auxiliaries of magnetism properly so called, required no direct experiments, since the principal agent, since magnetism itself, had disappeared. Bailly, therefore, confined himself, in this respect, to anatomical and physiological considerations, remarkable for their clearness and precision. We read also, with a lively interest, in his report, some ingenious reflections on the effects of imitation in those assemblages of magnetised people. Bailly compares them to those of theatrical representations. He says: "Observe how much stronger the impressions are when there are a great many

spectators, and especially in places where there is the liberty of applauding. This sign of particular emotions produces a general emotion, participated in by every body according to their respective susceptibility. This is also observed in armies on the day of battle, when the enthusiasm of courage, as well as panic terrors, propagate themselves with so much rapidity. The sound of the drum and of military music, the noise of the cannon, of the musketry, the cries, the disorder, stagger the organs, impart the same movement to men's minds, and raise their imaginations to a similar degree. In this unity of intoxication, an impression once manifested becomes universal; it encourages men to charge, or determines men to fly." Some very curious examples of imitation close this portion of Bailly's report.

The commissioners finally examined whether these convulsions, occasioned by the imagination or by magnetism, could be useful in curing or easing the suffering persons. The reporter said: "Undoubtedly, the imagination of sick people often influences the cure of their maladies very much . . . . There are cases in which every thing must first be disordered, to enable us to restore order . . . . but the shock must be unique . . . . . whereas in the public treatment by magnetism . . . . . . the habit of the crises cannot but be injurious."

This thought related to the most delicate considerations. It was developed in a report addressed to the king personally. This report was to have remained secret, but it was published some years since. It should not be regretted: the magnetic treatment, regarded in a certain point of view, pleased sick people much; they are now aware of all its dangers.

In conclusion, Bailly's report completely upsets an accredited error. This was an important service, nor was it the only one. In searching for the imaginary cause of animal magnetism, they ascertained the real power that man can exert over man, without the immediate and demonstrable intervention of any physical agent; they established that "the most simple actions and signs sometimes produce most powerful effects; that man's action on the imagination may be reduced to an art . . . . at least in regard to persons who have faith." This work finally showed how our faculties should be experimentally studied; in what way psychology may one day come to be placed among the exact sciences.

I have always regretted that the commissioners did not judge it expedient to add a historical chapter to their excellent work. The immense erudition of Bailly would have given to it an inestimable value. I figure to myself also, that in seeing the Mesmeric practices that have now been in use during upwards of two thousand years, the public would have asked itself whether so long

an interval of time had ever been required to push a good and useful thing forward into estimation. By circumscribing himself to this point of view, a few traits would have sufficed.

Plutarch, for example, would have come to the aid of the reporter. He would have showed him Pyrrhus curing complaints of the spleen, by means of frictions made with the great toe of his right foot. Without giving oneself up to a wild spirit of interpretation, we might be permitted to see in that fact the germ of animal magnetism. I admit that one circumstance would have rather unsettled the savant: this was the white cock that the King of Macedon sacrificed to the gods before beginning these frictions.

Vespasian, in his turn, might have figured among the predecessors of Mesmer, in consequence of the extraordinary cures that he effected in Egypt by the action of his foot. It is true that the pretended cure of an old blindness, only by the aid of a little of that emperor's saliva, would have thrown some doubt on the veracity of Suetonius.

Homer and Achilles are not too far back but we might have invoked their names. Joachim Camerarius, indeed, asserted having seen, on a very ancient copy of the Iliad, some verses that the copyists sacrificed because they did not understand them, and in which the poet alluded, not to the heel of Achilles (its celebrity has been well established these three thousand years), but to the medical properties possessed by the great toe of that same hero's right foot.

What I regret most is, the chapter in which Bailly might have related how certain adepts of Mesmer's had the hardihood to magnetise the moon, so as, on a given day, to make all the astronomers devoted to observing that body fall into a syncope; a perturbation, by the way, that no geometer, from Newton to Laplace, had thought of.

The work of Bailly gave rise to trouble, spite, and anger, among the Mesmerists. It was for many months the target for their combined attacks. All the provinces of France saw refutations of the celebrated report arise: sometimes under the form of calm discussions, decent and moderate; but generally with all the characteristics of violence, and the acrimony of a pamphlet.

It would be labour thrown away now to go to the dusty shelves of some special library, to hunt up hundreds of pamphlets, even the titles of which are now completely forgotten. The impartial analysis of that ardent controversy does not call for such labour; I believe at least that I shall attain my aim, by concentrating my attention on two or three writings which, by the strength of the arguments, the merit of the style, or the reputation of their authors have left some trace in men's minds.

In the first rank of this category of works we must place th elegant pamphlet published by Servan, under the title of, *Doubt of a Provincial, proposed to the Gentlemen Medical Commissioner commanded by the King to examine into Animal Magnetism.*

The appearance of this little work of Servan's was saluted in the camp of the Mesmerists with cries of triumph and joy. Undecided minds fell back into doubt and perplexity. Grimm wrote in Nov., 1784: "No cause is desperate. That of magnetism seemed as if it must fall under the reiterated attacks of medicine, of philosophy, of experience and of good sense . . . . . . Well, M. Servan, formerly the Attorney-General at Grenoble, has been proving that with talent we may recover from any thing, even from ridicule."

Servan's pamphlet seemed at the time the anchor of salvation for the Mesmerists. The adepts still borrow from it their principal arguments. Let us see, then, whether it has really shaken Bailly's report.

From the very commencing lines, the celebrated Attorney-General puts the question in terms deficient in exactness. If we believe him, the commissioners were called to establish a parallel between magnetism and medicine; "they were to weigh on both sides the errors and the dangers; to indicate with wise discernment what it would be desirable to preserve, and what to retrench, in the two sciences." Thus, according to Servan, the sanative art altogether would have been questioned, and the impartiality of the physicians might appear suspicious. The clever magistrate took care not to forget, on such an occasion, the eternal maxim, no one can be both judge and client. Physicians, then, ought to have been excepted.

Then there follows a legitimate homage to the non-graduated academicians, members of the commission: "Before Franklin and Bailly," says the author, "every knee must bend. The one has invented much, the other has discovered much; Franklin belongs to the two worlds, and all ages seem to belong to Bailly." But arming himself afterwards with more cleverness than uprightness, with these words of the reporter, "The commissioners, especially the doctors, made an infinity of experiments," he insinuates under every form that the commissioners accepted of a very passive line of conduct. Thus, putting aside the most positive declarations, pretending even to forget the name, the titles of the reporter, Servan no longer sees before him but one class of adversaries, regent doctors of the Faculty of Paris, and then he gives full scope to his satirical vein. He holds it even as an honour that they do not regard him as impartial. "The doctors have killed me; what it has pleased them to leave me of life is not worth, in truth, my

seeking a milder term . . . . For these twenty years I have always been worse through the remedies administered to me than through my maladies . . . . Even were animal magnetism a chimera, it should be tolerated; it would still be useful to mankind, by saving many individuals among them from the incontestable dangers of vulgar medicine . . . . . . I wish that medicine, so long accustomed to deceive itself, should still deceive itself now, and that the famous report be nothing but a great error . . . . ." Amidst these singular declarations, there are hundreds of epigrams still more remarkable by their ingenious and lively turn than by their novelty. If it were true, Gentleman, that the medical corps had ever tried, knowingly, to impose on the vulgar, to hide the uncertainty of their knowledge, the weakness of their theories, the vagueness of their conceptions, under an obscure and pedantic jargon, the immortal and laughable sarcasms of Molière would not have been more than an act of strict justice. In all cases everything has its day; now, towards the end of the eighteenth century, the most delicate, the most thorny points of doctrine were discussed with an entire good faith, with perfect lucidity, and in a style that placed many members of the faculty in the rank of our best speakers. Servan, however, goes beyond the limits of a scientific discussion, when, without any sort of excuse, he accuses his adversaries of being anti-mesmerists through esprit de corps, and, what is worse, through cupidity.

Servan is more in his element when he points out that the present best established medical theories occasioned at their birth prolonged debates; when he reminds us that several medicines have been alternately proscribed and recommended with vehemence: the author might even have more deeply undermined this side of his subject. Instead of some unmeaning jokes, why did he not show us, for example, in a neighbouring country, two celebrated physicians, Mead and Woodward, deciding, sword in hand, the quarrel that had arisen between them as to the purgative treatment of a patient? We should then have heard Woodward, pierced through and through, rolling on the ground, and drenched in blood, say to his adversary with an exhausted voice: "The blow was harsh, but yet I prefer it to your medicine!"

It is not truth alone that has the privilege of rendering men passionate. Such was the legitimate result of these retrospective views. I now ask myself whether, by labouring to put the truth of this aphorism in full light, the passionate advocate of Mesmerism showed proof of ability!

Gentlemen, let us put all these personal attacks aside, all these recriminations against science and its agents, who unfortunately had not succeeded in restoring the health of the morose magis-

trate. What remains then of his pamphlet? Two chapters, only two chapters, in which Bailly's report is treated seriously. The medical commissioners and the members of the Academy had not seen, in the real effects of Mesmerism anything more than was occasioned by imagination. The celebrated magistrate exclaims on this subject, "Any one hearing this proposition spoken of would suppose, before reading the report, that the commissioners had treated and cured, or considerably relieved by the force of imagination, large tumours, inveterate obstructions, gutta serenas, and strong paralyses." Servan admitted, in short, that magnetism had effected most wonderful cures. But there lay all the question. The cures being admitted, the rest followed as a matter of course.

However incredible these cures might be, they must be admitted, they said, when numerous witnesses certified their truth. Was it owing to chance that attestations were wanting for the miracles at the Cemetery of St. Médard? Did not the counsellor to the parliament, Montgeron, state, in three large quarto volumes, the names of a great multitude of individuals who protested on their honour as illuminati, that the tomb of the Deacon, Pâris, had restored sight to the blind, hearing to the deaf, strength to the paralytic; that in a twinkling it cured ailing people of gouty rheumatism, of dropsy, of epilepsy, of phthisis, of abscesses, of ulcers, &c.? Did these attestations, although many emanated from persons of distinction, from the Chevalier Folard, for example, prevent the convulsionists from becoming the laughingstock of Europe? Did they not see the Duchess of Maine herself laugh at their prowess in the following witty couplet?—

"A scavenger at the palace-gate
Who, his left heel being lame,
Obtained as a most special grace,
That his right should ail the same." *

Was not government, urged to the utmost, at last obliged to interfere, when the multitude, carrying folly to the extremest bounds, was going to try to resuscitate the dead? In short, do we not remember the amusing distich, affixed at the time to the gate of the Cemetery of St. Médard?—

"By royal decree, we prohibit the gods
To work any miracles near to these sods." †

Servan must have known better than any one that in regard to

* "Un décrotteur à la royale,
Du talon gauche estropié,
Obtint pour grace spéciale
D'être boiteux de l'autre pié."

† "De par le Roi, défense à Dieu
D'opérer miracle en ce lieu!"

estimony, and in questions of complex facts, quality always carries he day over mere numbers; let us add, that quality does not result either from titles of nobility, or from riches, nor from the social position, nor even from a certain sort of celebrity. What we must seek for in a witness is a calmness of mind and of feeling, a store of knowledge, and a very rare thing, notwithstanding the name it bears, common sense; on the other hand, what we must most avoid is the innate taste of some persons for the extraordinary, the wonderful, the paradoxical. Servan did not at all recollect these precepts in the criticism he wrote on Bailly's work.

We have already remarked that the Commissioners of the Academy and of the Faculty did not assert that the Mesmeric meetings were always ineffectual. They only saw in the crises the mere results of imagination; nor did any sort of magnetic fluid reveal itself to their eyes. I will also prove, that imagination alone generated the refutation that Servan gave to Bailly's theory. "You deny," exclaims the attorney-general, "you deny, gentlemen commissioners, the existence of the fluid which Mesmer has made to act such an important part! I maintain, on the contrary, not only that this fluid exists, but also that it is the medium by the aid of which all the vital functions are excited; I assert that imagination is one of the phenomena engendered by this agent; that its greater or less abundance in this or that among our organs, may totally change the normal intellectual state of individuals."

Everybody agrees that too great a flow of blood towards the brain produces a stupefaction of the mind. Analogous or inverse effects might evidently be produced by a subtle, invisible, imponderable fluid, by a sort of nervous fluid, or magnetic fluid (if this term be preferred), circulating through our organs. And the commissioners took good care not to speak on this subject of impossibility. Their thesis was more modest; they contented themselves with saying that nothing demonstrated the existence of such a fluid. Imagination, therefore, had no share in their report; but in Servan's refutation, on the contrary, imagination was the chief actor.

One thing that was still less proved, if possible, than any of those that we have been speaking of, is the influence that the magnetic fluid of the magnetiser might exert on the magnetised person.

In magnetism, properly so called, in that which physicists have studied with so much care and success, the phenomena are constant. They are reproduced exactly under the same conditions of form, of duration, and of quantity, when certain bodies, being present to each other, find themselves exactly in the same relative positions. That is the essential and necessary character of all

purely material and mechanical action. Was it thus in the pretended phenomena of animal magnetism? In no way. To-day the crises would occur in the space of some seconds; to-morrow they may require several entire hours; and finally, on another day, other circumstances remaining the same, the effect would be positively null. A certain magnetiser exercised a brisk action on a certain patient, and was absolutely powerless on another, who, on the contrary, entered into a crisis under the earliest efforts of a second magnetiser. Instead of one or two universal fluids, there must then, to explain the phenomena, be as many distinct fluids and constantly acting, as there exist animated or inanimate beings in the world.

The necessity of such a hypothesis evidently upset Mesmerism from its very foundations; yet the illuminati did not judge thus. All bodies became a focus of special emanations, more or less subtle, more or less abundant, and more or less dissimilar. So far the hypothesis found very few contradictors, even among rigorous minds; but soon these individual corporeal emanations were endowed, relatively towards those (without the least appearance of proof), either with a great power of assimilation, or with a decided antagonism, or with a complete neutrality; but they pretended to see in these occult qualities the material causes of the most mysterious affections of the soul. Oh! then doubt had a legitimate right to take possession of all those minds that had been taught by the strict proceedings of science not to rest satisfied with vain words. In the singular system that I have been explaining, when Corneille says,—

> "There are some secret knots, some sympathies,
> By whose relations sweet assorted souls
> Attach themselves the one to the other . . ." *

and when the celebrated Spanish Jesuit Balthazar Gracian spoke of the natural relationship of minds and hearts, both the one and the other alluded, assuredly without suspecting it, to the mixture, penetration, and easy crossing of two atmospheres.

"I love thee not, Sabidus," wrote Martial, "and I know not why; all that I can tell thee is, that I love thee not." Mesmerists would soon have relieved the poet from his doubts. If Martial loved not Sabidus, it was because their atmospheres could not intermingle without occasioning a kind of storm.

Plutarch informs us that the conqueror of Arminius fainted at the sight of a cock. Antiquity was astonished at this pheno-

* " Il est des nœuds secrets, il est des sympathies,
Dont par les doux rapports les âmes assorties
S'attachent l'une à l'autre."

menon. What could be more simple, however? the corporeal emanations of Germanicus and of the cock exercised a repulsive action the one on the other.

The illustrious biographer of Cheronea declares, it is true, that the presence of the cock was not requisite, that its crowing produced exactly the same effect on the adopted son of Tiberius. Now, the crowing may be heard a long way off; the crowing, then, would seem to possess the power of transporting the corporeal emanations of the king of the lower court with great rapidity through space. The thing may appear difficult to believe. As for myself, I think it would be puerile to stop at such a difficulty; have we not leaped high over other difficulties far more embarrassing?

The Maréchal d'Albret was still worse off than Germanicus: the atmosphere that made him fall into a syncope exhaled from the head of a wild boar. A live, complete, whole wild boar produced no effect, but on perceiving the head of the animal detached from the body, the Maréchal was struck as if with lightning. You see, Gentlemen, to what sad trials military men would be exposed, if the Mesmerian theory of atmospheric conflicts were to regain favour. We ought to be carefully on our guard against a ruse de guerre, of which no one till then had ever thought,—that is, against cocks, wild boars, &c.,—for through them an army might suddenly be deprived of its commander-in-chief. "It would also be requisite not to entrust command," Montaigne says, "to men who would fly from apples more than from arquebusades."

It is not only amongst the corpuscular emanations of living animals that the Mesmerists asserted conflicts to occur. They unhesitatingly extended their speculations to dead bodies. Some ancients dreamt that a catgut cord made of a wolf's intestines would never strike in unison with one made from a lamb's intestine; a discord of atmospheres renders the phenomenon possible. It is still a conflict of corporeal emanations that explains the other aphorism of an ancient philosopher: "The sound of a drum made with a wolf's skin takes away all sonorousness from a drum made with a lamb's skin."

Here I pause, Gentlemen. Montesquieu said: "When God created the brains of human beings, he did not intend to guarantee them."

To conclude: Servan's witty, piquant, agreeably written pamphlet was worthy under this triple claim of the reception with which the public honoured it; but it did not shake, in any one part, the lucid, majestic, elegant report by Bailly. The magistrate of Grénoble has said, that in his long experience he had met men accustomed to reflect without laughing, and other men who only

wished to laugh without reflecting. Bailly thought of the first cla: when he wrote his memorable report. *The Doubts of the Provincia man* were destined only for the other class.

It was also to these light and laughing souls that Servan exclu sively addressed himself some time after, if it be true that th *Queries of the young Doctor Rhubarbini de Purgandis* were writte by him.

Rhubarbini de Purgandis sets to work manfully. In his opinio the report by Franklin, by Lavoisier, by Bailly is, in the scientifi life of those learned men, what the *Monades* were for Leibnitz the *Whirlwinds* for Descartes, the *Commentary on the Apocalyps* for Newton. These examples may enable us to judge of the rest and render all farther refutation unnecessary.

Bailly's report destroyed root and branch the ideas, the systems the practices of Mesmer and of his adepts. Let us add sincerely tha we have no right to appeal to him in regard to modern somnambu lism. The greater portion of the phenomena now grouped arounc that name were neither known nor announced in 1783. A magne tizer certainly says the most improbable thing in the world, wher he affirms that a given individual in the state of somnambulism car see everything in the most profound darkness, that he can reac through a wall, and even without the help of his eyes. But the improbability of these announcements does not result from the celebrated report, for Bailly does not mention such marvels, neither in praise nor dispraise; he does not say one word about them. The physicist, the doctor, the merely curious man who gives himself up to experiments in somnambulism, who thinks he must examine whether, in certain states of nervous excitement, some individuals are really endowed with extraordinary faculties; with the faculty, for example, of reading with their stomach, or with their heel; people who wish to know exactly up to what point the phenomena so boldly asserted by the magnetizers of our epoch may be within the domain of rogues and sharks; all such people, we say, do not at all deny the authority of the subject in question nor do they put themselves really in opposition to the Lavoisiers, the Franklins, or the Baillys; they dive into an entirely new world, of which those illustrious learned men did not even suspect the existence.

I cannot approve of the mystery adopted by some grave learned men, who, in the present day, attend experiments on somnambulism. Doubt is a proof of diffidence, and has rarely been inimical to the progress of science. We could not say the same of incredulity. He who, except in pure mathematics, pronounces the word *impossible,* is deficient in prudence. Reserve is especially requisite wher we treat of animal organization.

Our senses, notwithstanding twenty-four centuries of study, observations, and researches, are far from being an exhausted subject. Take, for example, the ear. A celebrated natural philosopher, Wollaston, occupied himself with it; and immediately we learn, that with an equal sensibility as regards the low notes a certain individual can hear the highest tones, whilst another cannot hear them at all; and it becomes proved that certain men, with perfectly sound organs, never heard the cricket in the chimney corner, yet did not doubt but that bats occasionally utter a piercing cry; and attention being once awakened to these singular results, observers have found the most extraordinary differences of sensibility between their right ear and their left ear, &c.

Our vision offers phenomena not less curious, and an infinitely vaster field of research. Experience has proved, for example, that some people are absolutely blind to certain colours, as red, and enjoy perfect vision relatively to yellow, to green, and to blue. If the Newtonian theory of emission be true, we must irrevocably admit that a ray ceases to be light as soon as we diminish its velocity by one ten thousandth part. Thence flow those natural conjectures, which are well worthy of experimental examination: all men do not see by the same rays; decided differences may exist in this respect in the same individual during various nervous states; it is possible that the calorific rays, the dark rays of one person, may be the luminous rays of another person, and reciprocally; the calorific rays traverse some substances freely, which are therefore called diathermal, these substances, thus far, had been called opaque, because they transmit no ray commonly called luminous; now the words opaque and diathermal have no absolute meaning. The diathermals allow those rays to pass through which constitute the light of one man; and they stop those which constitute the light of another man. Perhaps in this way the key of many phenomena might be found, that till now have remained without any plausible explanation.

Nothing, in the marvels of somnambulism, raised more doubts than an oft-repeated assertion, relative to the power which certain persons are said to possess in a state of crisis, of deciphering a letter at a distance with the foot, the nape of the neck, or the stomach. The word *impossible* in this instance seemed quite legitimate. Still, I do not doubt but some rigid minds would withhold it after having reflected on the ingenious experiments by which Moser produces, also at a distance, very distinct images of all sorts of objects, on all sorts of bodies, and in the most complete darkness.

When we call to mind in what immense proportion electric

or magnetic actions increase by motion, we shall be less inclined to deride the rapid actions of magnetizers.

In here recording these developed reflections, I wished to show that somnambulism must not be rejected *à priori*, especially by those who have kept well up with the recent progress of the physical sciences. I have indicated some facts, some resemblances, by which magnetizers might defend themselves against those who would think it superfluous to attempt new experiments, or even to see them performed. For my part, I hesitate not to acknowledge it, although, notwithstanding the possibilities that I have pointed out, I do not admit the reality of the readings, neither through a wall, nor through any other opaque body, nor by the mere intromission of the elbow, or the occiput,— still I should not fulfil the duties of an academician if I refused to attend the meetings where such phenomena were promised me, provided they granted me sufficient influence as regards the proofs, for me to feel assured that I was not become the victim of mere jugglery.

Nor did Franklin, Lavoisier, or Bailly believe in Mesmeric magnetism before they became members of the Government Commission, and yet we may have remarked with what minute and scrupulous care they varied the experiments. True philosophers ought to have constantly before their eyes those two beautiful lines:

> "To suppose that everything has been discovered is a profound error:
> It is mistaking the horizon for the limits of the world." *

ELECTION OF BAILLY INTO THE ACADEMY OF INSCRIPTIONS.

In speaking of the pretended identity of the Atlantis, or of the kingdom of Ophir under Solomon with America, Bailly says, in his fourteenth letter to Voltaire: "Those ideas belonged to the age of learned men, but not to the philosophic age." And elsewhere (in the twenty-first letter) we read these words: "Do not fear that I shall fatigue you by heavy erudition." To have supposed that erudition could be heavy and be deficient in philosophy, was for certain people of a secondary order an unpardonable crime. And thus we saw men, excited by a sentiment of hate, arm themselves with a critical microscope, and painfully seek out imperfections in the innumerable quotations with which Bailly had strengthened himself. The harvest was not abundant; yet, these eager ferrets succeeded in discovering some weak points, some interpretations that might be contested. Their joy then knew no bounds. Bailly was treated with haughty disdain: "His literary

* "Croire tout découvert est un erreur profonde:
C'est prendre l'horizon pour les bornes du monde."

erudition was very superficial; he had not the key of the sanctuary of antiquity; he was everywhere deficient in languages."

That it might not be supposed that these reproaches had any reference to Oriental literature, Bailly's adversaries added: "that he had not the least tincture of the ancient languages; that he did not know Latin."

He did not know Latin? And do you not see, you stupid enemies of the great Astronomer, that if it had been possible to compose such learned works as *the History of Astronomy*, and *The Letters on the Atlantis*, without referring to the original texts, by using translations only, you would no longer have preserved any importance in the literary world. How is it that you did not remark, that by despoiling Bailly (and very arbitrarily) of the knowledge of Latin, you showed the inutility of studying that language to become both one of your best writers, and one of the most illustrious philosophers of the age?

The Academy of Inscriptions and Belles Lettres, far from participating in these puerile rancours, in the blind prejudices of some lost children of erudition, called Bailly to its bosom in 1785. Till then, Fontenelle alone had had the honour of belonging to the three great Academies of France. Bailly always showed himself very proud of a distinction which associated his name in an unusual manner with that of the illustrious writer, whose eulogies contributed so powerfully to make science and scientific men known and respected.

Independently of this special consideration, Bailly, as member of the French Academy, could all the better appreciate the suffrages of the Academy of Inscriptions, since there existed at that time between those two illustrious Societies a strong and inexplicable feeling of rivalry. This had even proceeded so far, that by a most solemn deliberation of the Academy of Inscriptions, any of its members would have ceased to belong to it, would have been irrevocably expelled, if they had even only endeavoured to be received into the French Academy; and the king having annulled this deliberation, fifteen Academicians bound themselves by oath to observe all its stipulations notwithstanding; furthermore, in 1783, Choiseul Gouffier, who was accused of having adhered to the principles of the fifteen confederates, and then of having allowed himself to be nominated by the rival Academy, was summoned by Anquetil to appear before the Tribunal of the Marshals of France for having broken his word of honour.

But, I may be allowed here to remark, superior men have always had the privilege of upsetting, by the mere influence of their name, the obstacles that routine, prejudices, and jealousy wished to oppose to the progress and the union of souls.

REPORT ON THE HOSPITALS.

Scientific tribunals, which should pronounce in the first instance while awaiting the definitive judgment of the public, were one of the requisites of our epoch; and thus, without any formal prescription of its successive regulations, the Academy of Sciences has been gradually led on to appoint committees to examine all the papers that have been presented to it, and to pronounce on their novelty, merit, and importance. This labour is generally an ungrateful one, and without glory, but talent has immense privileges: entrust Bailly with those simple Academical Reports, and their publication becomes an event.

M. Poyet, architect and comptroller of buildings in Paris, presented to Government in the course of the year 1785, a paper wherein he strove to establish the necessity of removing the Hôtel Dieu, and building a new hospital in another locality. This document, submitted by order of the king to the judgment of the Academy, gave rise, directly or indirectly, to three deliberations. The Academic Commissioners were; Lassone, Tenou, Tillet, Darcet, Daubenton, Bailly, Coulomb, Laplace, and Lavoisier. It was Bailly, however, who constantly held the pen. His reports have been honoured with a great and just celebrity. The progress of science would now perhaps allow of some modification being made in the ideas of the illustrious commissioners. Their views on warming-rooms, on their size, on ventilation, on general health, might, for example, receive some real ameliorations; but nothing could add to the sentiments of respect inspired by Bailly's work. What clearness of exposition! What neatness, what simplicity of style! Never did a writer put himself more completely out of view; never did a man more sincerely seek to make the sacred cause of humanity triumph. The interest that Bailly takes in the poor is deep, but always exempt from parade; his words are moderate, full of gentleness, even where hasty feelings of anger and indignation would have been legitimate. Of anger and of indignation! Yes, Gentlemen; listen, and decide!

I have cited the names of the commissioners. At no time, and in no country, could more virtue and learning have been united. These select men, regulating themselves in this respect according to the most common logic, felt that the task of pronouncing on a reform of the Hôtel Dieu imposed on them the necessity of examining that establishment. "We have asked," said their interpreter, "we have asked the Board of Administration to permit us to see the hospital in detail, and accompanied by some one who could guide and instruct us . . . . . we required to know several particulars; we asked for them, but we obtained nothing."

We have obtained nothing! These are the sad, the incredible

words, that men so worthy of respect are obliged to insert in the first line of their report!

What then was the authority that allowed itself to be so deficient in the most usual respect towards commissioners invested with the confidence of the King, the Academy, and the Public? This authority consisted of several administrators (the type of them, it is said, is not quite lost), who looked upon the poor as their patrimony, who devoted to them a disinterested but unproductive activity; who were impatient at any amelioration, the germ of which had not developed itself either in their own heads, or in those of certain men, philanthropic by nature, or by the privilege of their station. Ah! if by enlightened and constant care that vast asylum, opened to poverty and sickness, near Notre-Dame, had been then conducted, now sixty years ago, only in a tolerable way, we should have understood how, in taking human nature into consideration, the promoters of this great benefit would have repelled an examination that seemed to throw a doubt on their zeal and on their good sense. But alas! let us take from Bailly's work a few traits of the moderate and faithful picture that he drew of the Hôtel Dieu, and you shall decide, Gentlemen, whether the susceptibility of the administrators was authorised; whether, on the contrary, they ought not themselves to have anticipated the unhoped-for help from the king's power, united to science, which was now offered to them; whether by retarding certain ameliorations by a single day, they did not commit the crime of lèse-humanity.

In 1786, infirmities of all sorts were treated at the Hôtel Dieu: surgical maladies, chronic maladies, contagious maladies, female diseases, infantine diseases, &c. Everything was admitted, but all presented an inevitable confusion.

A patient on arriving was often laid in the bed and in the sheets of a man who had had the itch, and had just died.

The department reserved for madmen being very confined, two were put to sleep together. Two madmen in the same sheets! Nature revolts at the very thought of it.

In the ward of St. Francis, reserved exclusively for men having the small-pox, there were sometimes, for want of other space, as many as six adults or eight children in a bed not a mètre and a half wide.

The women attacked with this frightful disease were mixed in the ward of St. Monique with others who had only a simple fever, and the latter fell an inevitable prey to the hideous contagion, in the very place where, full of confidence, they had hoped to recover their health.

Women with child, women in their confinement, were equally crowded, pell-mell, on narrow and infected truckle-beds.

Nor let it be supposed that I have borrowed from Bailly's Report some purely exceptional cases, belonging to those cruel times, when whole populations, suffering under some epidemic, were tried beyond all human anticipation. In their usual state, the beds of the Hôtel Dieu, which were not a mètre and a half wide, contained four, and often six patients; they were placed alternately head and feet, the feet of one touching the shoulders of the next; each had only for his share of space 25 centimetres (9 inches); now, a man of medium size, lying with his arms close to his body, is 48 centimetres (16 inches) broad at the shoulders. The poor patients then could not keep within the bed but by lying on their side perfectly immovable; no one could turn without pushing, without waking his neighbour; they therefore used to agree, as far as their illness would allow, for some of them to remain up part of the night in the space between the beds, whilst the others slept; and when the approaches of death nailed these unfortunate people to their place, did they not energetically curse that help, which in such a situation could only prolong their painful agony.

But it was not only that beds thus placed were a source of discomfort, of disgust; that they prevented rest and sleep; that an insupportable heat occasioned and propagated diseases of the skin and frightful vermin; that the fever patient bedewed his neighbours with his profuse perspirations; and that in the critical moment he might be chilled by contact with those whose hot fit would occur later, &c. Still more serious effects resulted from the presence of many sick in the same bed; the food, the medicines, intended for one person, often found their way to another. In short, Gentlemen, in those beds of multiple population, the dead often lay for hours, and sometimes whole nights, intermingled with the living. The principal charitable establishment in Paris thus offered those dreadful coincidences, that the poets of Rome, that ancient historians have represented under King Mezentius, as the utmost extreme of barbarism.

Such was, Gentlemen, the normal state of the old Hôtel Dieu. One word, one word only, will suffice to tell what was the exceptional state: they placed some patients on the tops or testers of those same beds, where we have found so much suffering, so many authorised maledictions.

Now, Gentlemen, let us, together with our fellow academician, cast a glance on the ward of surgical operations.

This ward was full of patients. The operations were performed in their presence. Bailly says, "We see there the preparations for the torment; there are heard the cries of the tormented. He

who has to suffer the next day has before him a picture of his own future sufferings; he who has passed through this terrible trial, must be deeply moved at those cries so similar to his own, and must feel his agonies repeated; and these terrors, these emotions, he experiences in the midst of the progress of inflammation or suppuration, retarding his recovery, and at the hazard of his life." . . . . . "To what purpose," Bailly justly exclaims, "would you make an unfortunate man suffer, if there is not a probability of saving him, and unless we increase that probability by all possible precautions?"

The heart aches, the mind becomes confused, at the sight of so much misery; and yet this hospital, so little in harmony with its intended purpose, still existed sixty years ago. It is in a capital, the centre of the arts, of knowledge, of polished manners; it is in an age renowned for the development of public wealth, for the progress of luxury, for the ruinous creation of a crowd of establishments devoted to amusements, to worldly and futile pleasures; it is by the side of the palace of an opulent archbishop; it is at the gate of a sumptuous cathedral, that the unfortunate, under the deceitful mask of charity, underwent such dreadful tortures. To whom should we impute the long duration of this vicious and inhuman organisation?

To the professors of the art? No, no, Gentlemen! By an inconceivable anomaly the physicians, the surgeons, never obtained more than a secondary, a subordinate influence over the administration of the hospitals. No, no, the sentiments of the medical body for the poor could not be doubted, at an epoch and in a country where Dr. Anthony Petit thus answered the irritated queen, Marie Antoinette, "Madam, if I came not yesterday to Versailles, it was because I was attending the lying-in of a peasant, who was in the greatest danger. Your Majesty errs, however, in supposing that I neglect the Dauphin for the poor; I have hitherto treated the young child with as much attention and care as if he had been the son of one of your grooms."

Preference was granted to the most suffering, to those in most danger, disregarding rank and fortune; such was, you see, Gentlemen, the sublime rule of the French Medical Corps; and such is still its gospel. I want no other proof of it than those admirable words addressed by our fellow labourer Larrey, to his friend Tanchou, when wounded at the Battle of Montmirail: "Your wound is slight, sir; we have only room and straw in this ambulance for serious wounds. They will take you into that stable."

The medical corps could not, therefore, with any reason be accused or suspected in regard to the old Hôtel Dieu of Paris.

If economy be invoked, I find an answer quite à-propos in

Bailly: the daily allowance for the patients at the Hôtel Dieu was notably higher than in other establishments in the capital more charitably organized.

Would any one go so far as to assert that the sick condemned to seek refuge in the hospitals, having their sensibilities blunted by labour, by misery, by their daily sufferings, would but faintly feel the effects of the horrible arrangements that the old Hôtel Dieu revealed to all clear-sighted people? I will quote from the report of our colleague; "The maladies continue nearly double the time at the Hôtel Dieu, compared with those at the Charité: the mortality there is also nearly double! . . . . All the trepanned die in that hospital; whilst this operation is tolerably successful in Paris, and still more so at Versailles."

The maladies continue double the time! The mortality there is double! All those who are trepanned die! The lying-in women die in a frightful proportion, &c. These are the sinister words that strike the eye periodically in the statements of the Hôtel Dieu; and yet, let us repeat it, years passed away, and nothing was altered in the organisation of the great hospital! Why persist in remaining in a condition that so openly wounds humanity? Must we, together with Cabanis, who also abused the old Hôtel Dieu severely, "must we exclaim, that abuses known by all the world, against which every voice is raised, have secret supporters who know how to defend them, in a manner to tire out well-meaning people? Must we speak of false characters, perverse hearts, that seemed to regard errors and abuses as their patrimony?" Let us dare to acknowledge it, Gentlemen, evil is generally perpetrated in a less wicked manner: it is done without the intervention of any strong passion; by vulgar, yet all-powerful routine, and ignorance. I observe the same thought, though couched in the calm and cleverly circumspect language of Bailly: "The Hôtel Dieu has existed perhaps since the seventh century, and if this hospital is the most imperfect of all, it is because it is the oldest. From the earliest date of this establishment, good has been sought, the desire has been to adhere to it, and constancy has appeared a duty. From this cause, all useful novelties have with difficulty found admission; any reform is difficult; there is a numerous administration to convince; there is an immense mass to move."

The immensity of the mass, however, did not discourage the old Commissioners of the Academy. Let this conduct serve as an example to learned men, to administrators, who might be called upon to cast an investigating eye on the whole of our beneficent and humane establishments. Undoubtedly, the abuses, if any yet exist, have not individually anything to be compared to those to which Bailly's report did justice; but would it be impossible for

them to have sprung up afresh in the course of half a century, and that in proportion to their multiplicity, they should still make enormous and deplorable breaches in the patrimony of the poor?

I shall modify very slightly, Gentlemen, the concluding words of our illustrious colleague's report, and I shall not in the least alter their innate meaning, if I say, in finishing this long analysis: "Each poor man is now laid alone in a bed, and he owes it principally to the gifted, persevering, and courageous efforts of the Academy of Sciences. The poor man ought to know it, and the poor man will not forget it." Happy, Gentlemen, happy the academy that can adorn itself with such reminiscences!

REPORT ON THE SLAUGHTER-HOUSES.

An attentive glance at the past has been, in all ages and in all countries, the infallible means of rightly appreciating the present. When we direct this glance to the sanitary state of Paris, the name of Bailly will again present itself in the first line amongst the promoters of a capital amelioration, which I shall point out in a few words.

Notwithstanding the numerous acts of parliament, notwithstanding the positive police regulations, which dated back to Charles IX., to Henry III., to Henry IV., slaughter-houses still existed in the interior of the capital, in 1788; for instance, at l'Apport-Paris, La Croix-Rouge, in the streets of the Butcheries, Mont-Martre, Saint-Martin, Traversine, &c. &c. The oxen were consequently driven in droves through frequented parts of the town; enraged by the noise of the carriages, by the excitements of the children, by the attacks or barking of the wandering dogs, they often sought to escape, entered houses, or alleys, spread alarm everywhere, gored people, and committed great damage. Fetid gases exhaled from buildings too small and badly ventilated; the offal that had to be carried away gave out an insupportable smell; the blood flowed through the gutters of the neighbourhood, with other remains of the animals, and putrified there. The melting of tallow, an inevitable annexation of all slaughter-houses, spread around disgusting emanations, and occasioned a constant danger of fire.

So inconvenient, so repulsive a state of things, awakened the solicitude of individuals and of the public administration; the problem was submitted to our predecessors, and Bailly, as usual, became the reporter of the Academical Committee. The other members were Messrs. Tillet, Darcet, Daubenton, Coulomb, Lavoisier, and Laplace.

When Napoleon, wishing to liberate Paris from the dangerous and insalubrious results of internal slaughter-houses, decreed the

construction of the fine slaughter-houses known by everybody, he found the subject already well examined, exhibited in all its points of view, in Bailly's excellent work. "We ask," said the reporter of the Academical Commission in 1788, "we ask that the shambles be removed to a distance from the interior of Paris;" and these interior shambles have disappeared accordingly. Does it create surprise that it required more than fifteen years to obtain the grant of this most reasonable demand? I will further remark that unfortunately there was nothing exceptional in this; he who sows a thought in a field rank with prejudices, with private interests, and with routine, must never expect an early harvest.

BIOGRAPHIES OF COOK AND OF GRESSET.

The publication of the five quarto volumes of which *the History of Astronomy* consists, together with the two powerful *reports* that I have just described, had worn out Bailly. To relax and amuse his mind, he resumed the style of composition that had enchanted him in his youth; he wrote some biographies, amongst others, that of Captain Cook, proposed as a prize-subject by the Academy of Marseilles, and the Life of Gresset.

The biography of Gresset first appeared anonymously. This circumstance gave rise to a singular scene, which the author used to relate with a smile. I will here myself repeat the principal traits of it, if it be only to deter writers, whoever they may be, from launching their works into the world without affixing their names to them.

The Marchioness of Créqui was a lady in the high circles of society, to whom a copy of the eulogium of the author of *Vert-Vert* was presented as an offering. Some days after Bailly went to pay her a visit; did he hope to hear her speak favourably of the new work? I know not. At all events, our predecessor would have been ill rewarded for his curiosity.

"Do you know," said the great lady as soon as she saw him, "a Eulogy of Gresset recently published? The author has sent me a copy of it, without naming himself. He will probably come to see me; he may perhaps have come already. What could I say to him? I do not think any one ever wrote worse. He mistakes obscurity for profundity: it is the darkness before the creation."

Notwithstanding all Bailly's efforts to change the subject of the conversation, perhaps on account of those very efforts, the Marchioness rose, goes in search of the pamphlet, puts it into the author's hands, and begs of him to read aloud, if it be but the first page—quite enough, she said, to enable one to judge of the rest.

Bailly used to read remarkably well. I leave it to be guessed whether, on this occasion, he was able to exercise this talent.

Superfluous trouble! Madame de Créqui interrupted him at each sentence by the most disagreeable commentaries, by exclamations such as the following: "Detestable style!" "Confusion worse confounded!" and other similar amenities. Bailly did not succeed in extorting any indulgence from Madame de Créqui, when fortunately the arrival of another visitor put an end to this insupportable torture.

Two years after this, Bailly having become the first personage in the city, some booksellers collected all his opuscula and published them. This time the Marchioness, who had lost all recollection of the scene that I have been describing, overpowered the Mayor of Paris with compliments and felicitations on account of this same eulogy, which she had before treated with such inhuman rigour.

Such a contrast excited the mirth of the author. Still, might I dare to say so, Madame de Créqui was perhaps sincere on both occasions; had the exaggerations of praise and of criticism been put aside, it would not have been impossible to defend both opinions. The early pages of the pamphlet might appear embarrassed and obscure, whilst in the rest there might be found great refinement, elegance, and appreciations full of taste.

ASSEMBLY OF THE NOTABLES. — BAILLY IS NAMED FIRST DEPUTY OF PARIS — AND SOON AFTER DEAN OR SENIOR OF THE DEPUTIES OF THE COMMUNES.

The Assembly of the Notables had no other effect than to show in a stronger light the disorder of the finances, and the other wounds that were galling France. It was then that the Parliament of Paris asked for the convocation of the States General. This demand was unfavourably received by Cardinal de Brienne. Soon afterwards the convocation became a necessity, and Necker, now in the ministry, announced in the month of November, 1788, that it was decreed in Council, and that the king had even granted to the third estate a double representation, which had been so imprudently disputed by the courtiers.

The districts were formed on the king's convocation, the 21st of April, 1789. That day was the first day of Bailly's political life. It was on the 21st of April that the Citizen of Chaillot, entering the Hall of the *Feuillants,* imagined, he said, that "he breathed a new atmosphere," and regarded "as a phenomenon that he should have become something in the body-politic, merely from his being a citizen."

The elections were to be made in two gradations. Bailly was named first elector of his district. A few days after, at the general meeting, the Assembly called him to the Board in quality of Secretary. Thus it was our fellow-academician who, in the beginning,

drew up the celebrated *procès-verbal* of the meetings of the electors of Paris, so often quoted by the historians of the revolution.

Bailly also took an active part in drawing up the records of his district, and the records of the body of electors. The part he acted in these two capacities could not be doubtful, if we judge of it by the three following short quotations extracted from his memoirs: "The nation must remember that she is sovereign and mistress to order everything. . . . . It is not when reason awakes, that we should allege ancient privileges and absurd prejudices. . . . . I shall praise the electors of Paris who were the first to conceive the idea of prefacing the French Constitution with a declaration of the Rights of Man."

Bailly had always been so extremely reserved in his conduct and in his writings, that it was difficult to surmise under what point of view he would consider the national agitation of '89. Hence, at the very beginning, the Abbé Maury, of the French Academy, proposed to unite himself to Bailly, and that they should reside at Versailles and have an apartment in common between them. It is difficult to avoid a smile when one compares the conduct of the eloquent and impetuous Abbé with the categorical declarations, so distinct and so progressive, of the learned astronomer.

On Tuesday, the 12th of May, the general assembly of the electors proceeded to ballot for the nomination of the first deputy of Paris. Bailly was chosen.

This nomination is often quoted as a proof of the high intelligence, and of the wisdom of our fathers, two qualities which, since that epoch, must have been constantly on the decline, if we are to believe the blind Pessimists. Such an accusation imposed on me the duty of carrying the appreciation of this wisdom, of this intelligence that is held up against us, even to numerical correctness. The following is the result; the majority of the votes was 159; Bailly obtained 173; this was fourteen more than he required. If fourteen votes had changed sides the result would have been different. Was this an incident, I ask, to exclaim so much against?

Bailly showed himself deeply affected by this mark of the confidence with which he was regarded. His sensibility, his gratitude, did not prevent him however from recording in his memoirs the following *naïve* observation. "I observed in the Assembly of the Electors a great dislike for literary men, and for the academicians."

I recommend this remark to all studious men who by circumstances, or by a sense of duty, may be thrown into the whirlpool of politics. Perhaps I may yield to the temptation of developing

t, when I shall have to characterise Bailly's connection with his o-labourers in the first municipality of Paris.

The great question on the verification of the powers was lready strongly agitated, the day that Bailly and the other Deputies of Paris for the first time were able to go to Versailles; ur academician had only spoken once in that majestic assembly, viz. o induce the adoption of the method of voting by members being *eated* or *standing*,—when, on the 3rd of June, he was named Senior f the Deputies of the Communes (or Commons). Formerly, the ight of presiding in the third house of the kingdom belonged to he provost of the merchants. Bailly in his diffidence thought hat the assembly, in assigning the chair to him, had wished to compensate the capital for the loss of an old privilege. This consideration induced him to accept of a duty that he thought above his powers, — he who always depicted himself as timid to an extreme, and not possessing a facility of speaking.

Men's minds were more animated, more ardent in 1789 than those would admit who always see in the present a faithful image of the past. But calumny, that murderous arm of political party, already respected no position. Knowledge, loyalty, virtue, did not suffice to shelter any one from its poisoned darts. Bailly experienced it on the very day after his nomination to such an eminent post as President of the Communes.

On the 29th of May, the Communes had voted an address to the king on the constantly recurring difficulties that the nobility opposed to the union of the States General in one assembly. In order to carry out this most solemn deliberation, Bailly solicited an audience, in which the moderate and respectful expression of the anxiety of six hundred loyal deputies was to be presented to the monarch. In the midst of these strifes the Dauphin died. Without taking the trouble to consult dates, the court party immediately represented Bailly as a stranger to the commonest proprieties, and totally deficient in feeling; he ought, they said, to have respected the most allowable of griefs; his importunities had been barbarous.

I had imagined that such ridiculous accusations were no longer thought of; the categorical explanations that Bailly himself gave on this topic, seemed to me as if they would have sufficed to convince the most prejudiced. I was deceived, Gentlemen; the reproach of violence, of brutal insensibility, has just been repeated by the pen of a clever and a conscientious man. I will give his recital: "Scarcely two hours had elapsed since the royal child had breathed his last sigh, when Bailly, President of the Third Estate, insisted on admission to the king, who had prohibited any one being allowed to intrude upon him. But so positive was the

demand, that they were obliged to yield, and Louis XVI. exclaimed, 'There are then no fathers in that chamber of the Third Estate.' The chamber very much applauded this trait of brutal insensibility in Bailly, which they termed a trait of Spartan stoicism."

As many errors as words. The following is the truth. The illness of the Dauphin had not prevented the two privileged orders from being received by the king. This preference offended the Communes. They ordered the President to solicit an audience. He discharged his duty with great caution. All his proceedings were concerted with two ministers, Necker and M. de Barentin. The king answered, "It is impossible for me to see M. Bailly in the situation in which I am to-night, nor to-morrow morning, nor to fix a day for receiving the deputation of the Third Estate." The note ends with these words: "Show my note to M. Bailly for his vindication."

Thus, on the day of these events the Dauphin was not dead; thus the king was not obliged to yield, he did not receive Bailly; thus the chamber had no act of insensibility to applaud; thus Louis XVI. perceived so clearly that the President of the Communes was fulfilling the duties of his office, that he felt it requisite to give him an exoneration.

The death of the Dauphin happened on the 4th of June. As soon as the assembly of the Third Estate were informed of it, they charged the President, I quote the very words, "to report to their majesties the deep grief with which this news had penetrated the Communes."

A deputation of twenty members, having Bailly at their head, was received on the 6th. The President thus expressed himself: "Your faithful Communes are deeply moved by the circumstance in which your majesty has the goodness to receive their deputation, and they take the liberty to address to you the expression of all their regrets, and of their respectful sensibility."

Such language can, I think, be delivered without uneasiness to the appreciation of all good men.

Let us be correct; the Communes did not obtain at once the audience that they demanded on account of the difficulties of the ceremonial. They would have wished to make the Third Estate speak kneeling. "This custom," said M. de Barentin, "has existed from time immemorial, and if the king wished . . . ." "And if twenty-five millions of men do not wish it," exclaimed Bailly, interrupting the minister, "where are the means to force them?" "The two privileged orders," replied the Guard of the Seals, somewhat stunned by the apostrophe, "no longer require the Third Estate to bend the knee; but, after having formerly pos-

sessed immense privileges in the ceremonial, they limit themselve now to asking some difference. This difference I cannot find. "Do not take the trouble to seek for it," replied the Presiden hastily: "however slight the difference might be, the Commune will not suffer it."

This digression was required through a grave and recent erro The memory of Bailly will not suffer by it, since it has afforde me the opportunity of establishing, beyond any reply, that in ou fellow academician a noble firmness was on occasions allied t urbanity, mildness, and politeness. But what will be said of th puerilities which I have been obliged to recall, of the mean pretensions of the courtiers on the eve of an immense revolution? When the Greeks of the Lower Empire, instead of going on the ramparts valiantly to repel the attacks of the Turks, remained night and day collected around some sophists in their lyceums and academies, their sterile debates at least related to some intellectual questions; but at Versailles, there was nothing in action, on the part of two out of three orders, but the most miserable vanity.

By an express arrangement, decreed from the beginning, among the Members of the Communes, the Dean or President had to be renewed every week. Notwithstanding the incessant representations of Bailly, this legislative article was long neglected, so fortunate did the Assembly feel in having at their head this eminent man, who to undeniable knowledge, united sincerity, moderation, and a degree of patriotism not less appreciated.

He thus presided over the Third Estate on the memorable days that determined the march of our great revolution.

On the 17th of June, for instance, when the Deputies of the Communes, worn out with the tergiversations of the other two orders, showed that in case of need they would act without their concurrence, and resolutely adopted the title of National Assembly, — they provided against presumed projects of dissolution, by stamping as illegal all levies of contribution which were not granted by the Assembly.

Again, on the 20th of June, when the Members of the National Assembly, affronted at the Hall having been closed and their meetings suspended without an official notification, with only the simple form of placards and public criers, as if a mere theatre was in question, they assembled at a tennis-court, and "took an oath never to separate, but to assemble wherever circumstances might render it requisite, until the Constitution of the Kingdom should be established and confirmed on solid foundations."

Once more, Bailly was still at the head of his colleagues on the 23rd of June, when, by an inexcusable inconsistency, and which

perhaps was not without some influence on the events of that day, the Deputies of the Third Estate were detained a long time at the servants' door of the Hall of Meeting, and in the rain; while the deputies of the other two orders, to whom a more convenient and more suitable entrance had been assigned, were already in their places.

The account that Bailly gave of the celebrated royal meeting on the 23rd of June, does not exactly agree with that of most historians.

The king finished his speech with the following imprudent words: "I order you, Gentlemen, to separate immediately."

The whole of the nobility and a portion of the clergy retired; while the Deputies of the Communes remained quietly in their places. The Grand Master of the Ceremonies having remarked it, approaching Bailly said to him, "You heard the king's order, Sir?" The illustrious President answered, "I cannot adjourn the Assembly until it has deliberated on it." "Is that indeed your answer, and am I to communicate it to the king?" "Yes, Sir," replied Bailly, and immediately addressing the Deputies who surrounded him, he said, "It appears to me that the assembled nation cannot receive an order."

It was after this debate, at once both firm and moderate, that Mirabeau addressed from his place the well-known apostrophe to M. de Brézé. The President disapproved both of the basis and the form of it; he felt that there was no sufficient motive; for, said he, the Grand Master of the Ceremonies made use of no menace; he had not in any way insinuated that there was an intention to resort to force; he had not, above all, spoken of bayonets. At all events, there is an essential difference between the words of Mirabeau as related in almost all the Histories of the Revolution, and those reported by Bailly. According to our illustrious colleague the impetuous tribune exclaimed, "Go tell those who sent you, that the force of bayonets can do nothing against the will of the nation." This is, to my mind, much more energetic than the common version. The expression, "We will only retire by the force of bayonets!" had always appeared to me, notwithstanding the admiration conceded to it, to imply only a resistance which would cease on the arrival of a corporal and half-a-dozen soldiers.

Bailly quitted the chair of President of the National Assembly on the 2nd of July. His scientific celebrity, his virtue, his conciliating spirit, had not been superfluous in habituating certain men to see a member of the Communes preside over an assembly in which there was a prince of the blood, a prince of the church, the greatest lords of the kingdom, and all the high dignitaries of the clergy. The first person named to succeed to Bailly was the

Duke d'Orléans. After his refusal, the Assembly chose the Archbishop of Vienne (Pompignan).

Bailly recalls to mind with sensibility, in his memoirs, the testimonies of esteem that he obtained through his difficult and laborious presidency. The 3rd of July, on the proposition of the Duke de la Rochefoucauld and of the Archbishop of Bordeaux, the National Assembly sent a deputation to their illustrious ex-president, to thank him (these are the precise words) "for his noble, wise, and firm conduct." The electoral body of Bordeaux had been beforehand with these homages. The Chamber of Commerce of that town, at the same time, decided that the portrait of the great citizen should decorate their hall of meeting. The Academy of Sciences, the Academy of Inscriptions and Belles Lettres, did not remain insensible to the glory that one of their members had acquired in the career of politics, and testified it by numerous deputations. Finally, Marmontel, in the name of the French Academy, expressed to Bailly "how proud that assembly was to count among its members an Aristides that no one was tired of calling the Just."

I shall not excite surprise, I hope, by adding, after such brilliant testimonies of sympathy, that the inhabitants of Chaillot celebrated the return of Bailly amongst them by fêtes, and fireworks, and that even the curate of the parish and the churchwardens, unwilling to be surpassed by their fellow-citizens, nominated the historian of antediluvian astronomy honorary churchwarden. I will, at all events, repress the smile that might arise from such private reminiscences, by reminding the reader that a man's moral character is better appreciated by his neighbours, to whom he shows himself daily without disguise, than that of more considerable persons, who are only seen on state occasions, and in official costume.

BAILLY BECOMES MAYŌR OF PARIS.—SCARCITY.—MARAT DECLARES HIMSELF INIMICAL TO THE MAYOR.—EVENTS OF THE 6TH OF OCTOBER.

The Bastille had been taken on the 14th of July. That event, on which, during upwards of half a century, there have been endless discussions, on opposite sides, was characterised in the following way, in the address to the National Assembly, drawn up by M. Moreau de Saint Méry, in the name of the City Committee:—

"Yesterday will be for ever memorable by the taking of a citadel, consequent on the Governor's perfidy. The bravery of the people was irritated by the breaking of the word of honour. This act (the strongest proof that the nation who knows best how to obey, is jealous of its just liberties,) has been followed by incidents that from the public misfortunes might have been foreseen."

Lally Tollendal said to the Parisians, on the 15th of July:

"In the disastrous circumstances that have just occurred, we did not cease to participate in your griefs; and we have also participated in your anger: it was just."

The National Assembly solicited and obtained permission from the king on the 15th of July, to send a deputation to Paris, which they flattered themselves would restore order and peace in that great city, then in a convulsed state. Madame Bailly, always influenced by fear, endeavoured, though vainly, to dissuade her husband from joining the appointed deputies. The learned academician naïvely replied, "After a presidency that has been applauded, I am not sorry to show myself to my fellow-citizens." You see, Gentlemen, that Bailly always admits the future reader of his Posthumous Memoirs confidentially into his most secret feelings.

The deputation completed its mandate at the Town Hall, to the entire satisfaction of the Parisian populace; the Archbishop of Paris, its President, had already proposed to go in procession to the Cathedral to sing *Te Deum;* they were preparing to depart, when the Assembly, giving way to a spontaneous enthusiasm, with an unanimous voice, proclaimed Bailly Mayor of Paris, and Lafayette Commander-in-Chief of the National Guard, the creation of which had just been authorised.

The official minutes of the Municipality state, that on being thus unexpectedly named, Bailly bent forward to the Assembly, his eyes bathed in tears, and that amidst his sobs he could only utter a few unconnected words to express his gratitude. The Mayor's own recital differs very little from this official relation. Still I shall quote it as a model of sincerity and of modesty.

"I know not whether I wept, I know not what I said; but I remember well that I was never so surprised, so confused, and so beneath myself. Surprise adding to my usual timidity before a large assembly, I rose, I stammered out a few words that were not heard, and that I did not hear myself, but which my agitation, much more than my mouth, rendered expressive. Another effect of my sudden stupidity was, that I accepted without knowing what a burden I was taking on myself."

Bailly having become Mayor, and being tacitly accepted by the National Assembly, even from the 16th of July, availed himself of his intimacy with Vicq-d'Azyr, the Queen's physician, to persuade Louis XVI. to show himself to the Parisians. This advice was listened to. On the 17th the new magistrate addressed the king near the barrière de la Conférence, in a discourse that began thus: —

"I bring to your Majesty the keys of your good city of Paris. They are the same that were presented to Henry IV. He had

econquered his people, here the people have reconquered their :ing."

The antithesis: "he had reconquered his people, here the people ıave reconquered their king," was universally applauded. But ince then, it has been criticised with bitterness and violence. [he enemies of the Revolution have striven to discover in it an ntention of committing an outrage, to which the character of Bailly, ınd still more so the first glance at an examination of the rest ıf his discourse, give a flat contradiction. I will acknowledge ɟentlemen, I think that I have even a right to decline the epithet f "unfortunate," which one of our most respectable colleagues ı the French Academy has pronounced relative to this celebrated hrase, while doing justice at the same time to the sentiments of he author. The poison contained in the few words that I have uoted, was very inoffensive, since more than a year passed without any courtier, though furnished like a microscope with all the ıonarchical susceptibilities, beginning to suspect its existence.

The Mayor of Paris was at the Hôtel de Ville in the midst of hose same Parisian citizens who inspired him, a few months before, ⁄ith the mortifying reflection already quoted: "I remarked in the \ssembly of Electors a dislike to literary people and Academiians." The feeling did not appear to be changed.

The political movement in 1789, had been preceded by two 'ery serious physical perturbations which had great influence n the march of events. Every one is aware, that the excesively rigorous winter of 1788—89 was the cause of severe suf'erings to the people. But it may not be so generally known, hat on the 13th of July, 1788, a fall of hail of unprecedented ize and quantity, in a few hours completely ravaged the two ›arallel zones lying between the department of the Charente and he frontiers of the Pays-Bas, and that in consequence of this rightful hail, the wheat partly failed, both in the north and in the ⁄est of France, until after the harvest of 1789.

The scarcity was already severely felt, when Bailly on the 15th f July accepted the appointment of Mayor of Paris. That day, t had been ascertained, from an examination of the quantity of corn t the Market Hall and of the private stocks of the bakers, that he supply of grain and flour would be entirely exhausted in three ays. The next day, the 16th of July, all the overseers in the ictualling administration had disappeared. This flight, the natural onsequence of the terrible intimidation that hovered over those ⁄ho were in any way connected with the furnishing of provisions, nterrupted the operations which had been commenced, and exposed he city of Paris to famine.

Bailly, a magistrate of only one day's standing, considered that

the multitude understands nothing, hears nothing when bread fails; that a scarcity, either real or supposed, is the great promoter of riots; that all classes of the population grant their sympathy to whoever cries, *I am hungry;* that this lamentable cry soon unites individuals of all ages, of both sexes, of every condition, in one common sentiment of blind fury; that no human power could maintain order and tranquillity in the bosom of a population that dreads the want of food; he therefore resolved to devote his days and his nights to provisioning the capital; to deserve, as he himself said, the title of the *Father nourisher of the Parisians,*—that title of which he showed himself always so proud, after having painfully gained it.

Bailly day by day recorded in his Memoirs a statement of his actions, of his anxieties, and of his fears. It may be good for the instruction of the more fortunate administrators of the present epoch, to insert here a few lines from the journal of our colleague.

"18th August. Our provisions are very much reduced. Those of the morrow depend strictly on the arrangements made on the previous evening; and now amidst this distress, we learn that our flour-waggons have been stopped at Bourg-la-Reine; that some banditti are pillaging the markets in the direction of Rouen, that they have seized twenty waggons of flour that were destined for us; . . . that the unfortunate Sauvage was massacred at Saint Germain-en-Laye; . . . . that Thomassin escaped with difficulty from the fury of the populace at Choisy."

By repeating either these literal words, or something equivalent to them, for every day of distress throughout the year 1789, an exact idea may be formed of the anxieties that Bailly experienced from the morning after his installation as mayor. I deceive myself; to complete the picture we ought also to record the unreflecting and inconsiderate actions of a multitude of people whose destiny appeared to be, to meddle with everything and to spoil everything. I will not resist the wish to show one of these self-important men, starving (or very nearly so) the city of Paris.

"21st August. The store of victuals, Bailly says, was so scanty, that the lives of the inhabitants of Paris depended on the somewhat mathematical precision of our arrangements. Having learnt that a barge with eighteen hundred sacks of flour had arrived at Poissy, I immediately despatched a hundred waggons from Paris to fetch them. And behold, in the evening, an officer without powers and without orders, related before me, that having met some waggons on the Poissy road, he made them go back, because he did not think that there was a wharf for any loaded barge on the Seine. It would be difficult for me to describe the despair

ınd the anger into which this recital threw me. We were ›bliged to put sentinels at the bakers' doors!"

The despair and the anger of Bailly were very natural. Even ıow, after more than half a century, no one thinks without a hudder of that obscure individual who, from not believing that a oaded barge could get up to Poissy, was going, on the 21st August, 1789, to plunge the capital into bloody disorders.

By means of perseverance, devotedness, and courage, Bailly ucceeded in overcoming all the difficulties that the real scarcity, ınd the fictitious one, which was still more redoubtable, caused daily o arise. He succeeded, but his health from that epoch was leeply injured; his mind had undergone several of those severe hocks that we can never entirely recover from. Our colleague aid, "when I used to pass the bakers' shops during the scarcity, ınd saw them besieged by a crowd, my heart sunk within me; ınd even now that abundance has been restored to us, the sight ›f one of those shops strikes me with a deep emotion."

The administrative conflicts, the source of which lay in the very bosom of the Council of the Commune, daily drew from Bailly the following exclamation, a faithful image of his mind: *I have ceased to be happy.* The embarrassments that proceeded rom external sources touched him much less, and yet they were ar from contemptible. Let us surmount our repugnance, although ı reasonable one; let us cast a firm look on the sink where the ınworthy calumnies were manufactured, of which Bailly was for ome time the object.

Several years before our first revolution, a native of Neufchatel quitted his mountains, traversed the Jura, and lighted upon Paris. Without means, without any recognised talent, without eminence ›f any sort, repulsive in appearance, of a more than negligent leportment, it seemed unlikely that he should hope, or even lream, of success; but the young traveller had been told to have ull confidence, although a celebrated academician had not yet given that singular definition of our country, "France is the ıome of foreigners." At all events, the definition was not erroıeous in this instance, for soon after his arrival, the Neufchatelois vas appointed physician to the household of one of the princes of he royal family, and formed strict intimacies with the greater ›art of the powerful people about the court.

This stranger thirsted for literary glory. Amongst his early ›roductions, a medico-philosophical work figured in three volumes, elative to the reciprocal influences of the mind and the body. The author thought he had produced a *chef d'œuvre;* even Volaire was not thought to be above analysing it suitably; let us ıasten to say that the illustrious old man, yielding to the pressing

solicitations of the Duke de Praslin, one of the most active patrons of the Swiss doctor, promised to study the work and give his opinion of it.

The author was at the acmé of his wishes. After having pompously announced that the seat of the soul is in the *meninges* (cerebral membrane), could there be anything to fear from the liberal thinker of Ferney? He had only forgotten that the patriarch was above all a man of good taste, and that the book on the body and soul offended all the proprieties of life. Voltaire's article appeared. He began with this severe and just lesson—"We should not be prodigal of contempt towards others, and of esteem for ourselves, to such a degree as will be revolting to our readers." The end was still more overwhelming. "We see harlequin everywhere cutting capers to amuse the pit."

Harlequin had received a sufficient dose. Not having succeeded in literature, he threw himself upon the sciences.

On betaking himself to this new career, the doctor of Neufchatel attacked Newton. But unluckily his criticisms were directed precisely to those points wherein optics may vie in evidence with geometry itself. This time the patron was M. de Maillebois, and the tribunal the Academy of Sciences.

The Academy pronounced its judgment gravely, without inflicting a word of ridicule; for example, it did not speak of harlequin; but it did not therefore remain the less established that the pretended experiments, intended, it was said, to upset Newton's, on the unequal refrangibility of variously coloured rays, and the explanation of the rainbow, &c., had absolutely no scientific value.

Still the author would not allow himself to have been beaten. He even conceived the possibility of retaliation; and, availing himself of his intimacy with the Duke de Villeroy, governor of the second city in the kingdom, he got the Academy of Lyons to propose for competition all the questions in optics, which for several years past had been the subjects of its disquisitions; he even furnished the amount of the prizė out of his own pocket, under an assumed name.

The prize so longed for, and so singularly proposed, was not obtained, however, by the Duke de Villeroy's candidate, but by the astronomer Flaugergues. From that instant, the pseudo-physicist became the bitter enemy of the scientific bodies of the whole universe, of whoever bore the title of an academician. Putting aside all shame, he no longer made himself known in the field of natural philosophy, merely by imaginary experiments, or by juggleries; he had recourse to contemptible practices, with the object of throwing doubt upon the clearest and best proved prin-

ciples of science: for example, the metallic needles discovered by the academician Charles, and which the foreign doctor had adroitly concealed in a cake of resin, in order to contradict the common opinion of the electric non-conductibility of that substance.

These details were necessary. I could not avoid characterising the journalist who by his daily calumnies contributed most to undermine the popularity of Bailly. It was requisite besides, once for all, to strip him in this circle of the epithet of philosopher, with which men of the world, and even some historians, inconsiderately gratified him. When a man reveals himself by some brilliant and intelligent works, the public is pleased to find them united with good qualities of the heart. Nor should its joy be less hearty on discovering the absence of all intellectual merit in a man who had before shown himself despicable by his passions, or his vices, or even only by serious blemishes of character.

If I have not yet named the enemy of our colleague, if I have contented myself with recounting his actions, it is in order to avoid as much as I can the painful feeling that his name must raise here. Judge, Gentlemen, weigh, my scruples: the furious persecutor of Bailly, of whom I have been talking to you for some minutes, was Marat.

The revolution of '89 just occurred in time to relieve the abortive author, physiologist, and physicist from the intolerable position into which he had been thrown by his inability and his quackery.

As soon as the revolution had assumed a decided movement, great surprise was occasioned by the sudden transformations excited in the inferior walks of the political world. Marat was one of the most striking examples of these hasty changes of principles. The Neufchatel physician had shown himself a violent adversary to those opinions that occasioned the convocation of the assembly of Notables, and the national commotion in '89. At that time democratical institutions had not a more bitter or more violent censor. Marat liked it to be believed that in quitting France for England, he fled especially from the spectacle of social renovation which was odious to him. Yet a month after the taking of the Bastille, he returned to Paris, established a journal, and from its very beginning left far behind him, even those who, in the hope of making themselves remarkable, thought they must push exaggeration to its very farthest limits. The former connection of Marat with M. de Calonne was perfectly well known; they remembered these words of Pitt's: "The French must go through liberty, and then be brought back to their old government by licence;" the avowed adversaries of revolution testified by their conduct, by their votes, and even by their imprudent words, that

according to them, *the worst* was the only means of returning to what they call *the good;* and yet these instructive comparisons struck only eight or ten members of our great assemblies, so small a share has suspicion in the national character, so painful is distrust to French sincerity. The historians of our troubles themselves have but skimmed the question that I have just raised — assuredly a very important and very curious one. In such matters, the part of a prophet is tolerably hazardous; yet I do not hesitate to predict, that a minute study of the conduct and of the discourses of Marat, would lead the mind more and more to those chapters in a treatise on the chase, wherein we see depicted bad species of falcons and hawks, at first only pursuing the game by a sign from the master, and for his advantage; but by degrees taking pleasure in these bloody struggles, and entering on the sport at last with passion and for their own profit.

Marat took good care not to forget that during a revolution, men, naturally suspicious, act in their more immediate affairs so as to render those persons suspected whose duty it is to watch over them. The Mayor of Paris, the General Commandant of the National Guard, were the first objects, therefore, at which the pamphleteer aimed. As an academician, Bailly had an extra claim to his hate.

Among men of Marat's disposition, the wounds of self-love never heal. Without the hateful passions derived from this source, who would believe that an individual, whose time was divided between the superintendence of a daily journal, the drawing up of innumerable placards with which he covered the walls of Paris, together with the struggles of the Convention, the disputes not less fierce of the clubs; that an individual who, besides, had given himself the task of imposing an Agrarian law on the country, could find time to write the very long letters against the old official adversaries of his bad experiments, his absurd theories, his lucubrations devoid both of erudition and of talent: letters in which the Monges, the Laplaces, the Lavoisiers are treated with such an entire neglect of justice and of truth, and with such a cynical spirit, that my respect for this assembly prevents my quoting a single expression.

It was not then only the Mayor of Paris whom the pretended friend of the people persecuted; it was also the Academician Bailly. But the illustrious philosopher, the virtuous magistrate, gave no hold for positive and decided criminations. The hideous pamphleteer understood this well; and therefore he adopted vague insinuations, that allowed of no possible refutation, a method which, we may remark by the way, has not been without imitators. Marat exclaimed every day: "Let Bailly send in his accounts!"

and the most powerful figure of rhetoric, as Napoleon said, repetition, finally inspires doubts in a stupid portion of the public, in some feeble, ignorant, and credulous minds in the Council of the Commune; and the scrupulous magistrate wished, in fact, to send in his accounts. Here they are in two lines: Bailly never had the handling of any public funds. He left the Hôtel de Ville, after having spent there two thirds of his patrimony. If his functions had been long protracted, he would have retired completely ruined. Before the Commune assigned him any salary, the expenses of our colleague in charities already exceeded 30,000 livres.

That was, Gentlemen, the final result. The details would be more striking, and the name of Bailly would ennoble them. I could show our colleague entering only once with his wife, to regulate the furnishing of the apartments that the Commune assigned him; rejecting all that had the appearance of luxury or even of elegance; to replace sets of china by sets of earthenware, new carpets by the half-used ones of M. de Crosnes, writing tables of mahogany by writing tables of walnut, &c. But all this would appear an indirect criticism, which is far from my thoughts. From the same motives, I will not say, that inimical to all sinecures, of all plurality of appointments, when the functions are not fulfilled, the Mayor of Paris, since he no longer regularly attended the meetings of the National Assembly, no longer fingered the pay of a deputy, and that this was proved, to the great confusion of the idiots, whose minds had been disturbed by Marat's clamours. Yet I will record that Bailly refused all that in the incomes of his predecessors had proceeded from an impure source; as for example, the allowances from the lotteries, the amount of which was by his orders constantly paid into the coffers of the Commune.

You see, Gentlemen, that no trouble was required to show that the disinterestedness of Bailly was great, enlightened, dictated by virtue, and that it was at least equal to his other eminent qualities. In the series of accusations that I have extracted from the pamphlets of that epoch, there is one, however, as to which, all things considered, I will not attempt to defend Bailly. He accepted a livery from the city; on this point no blame was attached to him; but the colours of the livery were very gaudy. Perhaps the inventors of these bright shades had imagined, that the insignia of the first magistrate of the metropolis, in a ceremony, in a crowd, should like the light from a Pharos, strike even inattentive eyes. But these explanations regard those who would make of Bailly a perfectly rational being, a man absolutely faultless; I, although his admirer, I resign myself to admit that in a laborious life strewed with so many rocks, he committed the horrible crime, un-

pardonable let it be called, of having accepted from the Commune a livery of gaudy colours.

Bailly figured in the events of the month of October 1789, only by the unsuccessful efforts he made at Paris, to arrange with Lafayette how to prevent a great crowd of women from going to Versailles. When this crowd, considerably increased, returned on the 6th October very tumultuously escorting the carriages of the royal family, Bailly harangued the king at the Barrière de la Conférence. Three days after, he also complimented the Queen at the Tuileries in the name of the Municipal Council.

On retiring from the National Assembly, which he then called a Cavern of Anthropophagi, Lally Tollendal published a letter in which he found bitter fault with Bailly on account of these discourses. Lally was angry, recollecting that the day when the king re-entered his capital as a prisoner, surrounded by a very disrespectful crowd, and preceded by the heads of his body-guards, had appeared to Bailly a fine day!

If the two heads had been in the procession, Bailly becomes inexcusable; but the two epochs, or rather hours (to speak more correctly), have been confounded; the wretched men, who after a conflict with the body-guard, brought their barbarous trophies to Paris, left Versailles in the morning; they were arrested and imprisoned, by order of the municipality, as soon as they had entered the barriers of the capital. Thus the hideous circumstance reported by Lally was the dream of a wild imagination.

A GLANCE AT THE POSTHUMOUS MEMOIR OF BAILLY.

Bailly's Memoirs have thus far served me as a guide and check; now that this resource fails me, let us refer to his posthumous work.

I could only consult those Memoirs as far as they related to the public or private life of our colleague. Historians may consult them in a more general point of view. They will find some valuable facts in them, related without prejudice; ample matter for new and fruitful reflections on the way in which revolutions are generated, increase, and lead to catastrophes. Bailly is less positive, less absolute, less slashing, than the generality of his contemporaries, even respecting those events in which circumstances assigned to him the principal part to be acted; hence when he points out some low intrigue, in distinct and categorical terms, he inspires full confidence.

When the occasion will allow of it, Bailly praises with enthusiasm; a noble action fills him with joy; he puts it together and relates it with relish. This disposition of mind is sufficiently rare to deserve mention.

The day, still far off, when we shall finally recognise that our great revolution presented, even in the interior, even during the most cruel epochs, something besides anarchical and sanguinary scenes; the day when, like the intrepid fishermen in the Gulf of Persia and on the coasts of Ceylon, a zealous and impartial writer will consent to plunge head-foremost into the ocean of facts of all sorts, of which our fathers were witnesses, and exclusively seize the pearls, disdainfully rejecting the mud,—Bailly's Memoirs will furnish a glorious contingent to this national work. Two or three quotations will explain my ideas, and will show, besides, how scrupulously Bailly registered all that could shed honour on our country.

I will take the first fact from the military annals: a grenadier of the French Guard saves his commanding officer's life, although the people thought that they had great reason of complaint against him. "Grenadier, what is your name?" exclaimed the Duke de Châtelet, full of gratitude. The soldier replied, "Colonel, my name is that of all my comrades."

I will borrow the second fact from the civil annals: Stephen de Larivière, one of the electors of Paris, had gone on the 20th of July, to fetch Berthier de Sauvigny, who had been fatally arrested at Compiègne, on the false report that the Assembly of the Town Hall wished to prosecute him as intendant of the army, by which a few days before the capital had been surrounded. The journey was performed in an open cabriolet, amidst the insults of a misled population, who imputed to the prisoner the scarcity and bad quality of the bread. Twenty times, guns, pistols, sabres, would have put an end to Berthier's life, if twenty times, the member of the Commune of Paris had not voluntarily covered him with his body. When they reached the streets of the capital, the cabriolet had to penetrate through an immense and compact crowd, whose exasperation bordered on delirium, and who evidently wished to perpetrate the utmost extremities; not knowing which of the two travellers was the Intendant of Paris, they betook themselves to crying out, "let the prisoner take off his hat!" Berthier obeyed, but Larivière uncovered his head also at the same instant.

All parties would gain by the production of a work, that I desire to see most earnestly. For my part, I acknowledge, I should be sorry not to see in it the answer made to Francis II. by one of the numerous officers who committed the fault, so honestly acknowledged afterwards,—a fault that no one would commit now,—that of joining foreigners in arms. The Austrian prince, after his coronation, attempted at a review, to induce our countrymen to admire the good bearing of his troops, and finally exclaimed, "There are materials wherewith to crush the Sans-culottes."

"That remains to be seen!" instantly answered the émigré officer.

May these quotations lead some able writer to erect a monument still wanting to the glory of our country! There is in this subject, it seems to me, enough to inspire legitimate ambition. Did not Plutarch immortalise himself by preserving noble actions and fine sentiments from oblivion?

EXAMINATION OF BAILLY'S ADMINISTRATION AS MAYOR.

The illustrious Mayor of Paris had not the leisure to continue writing his reminiscences beyond the date of the 2nd of October, 1789. The analysis and appreciation of the events subsequent to that epoch will remain deprived of that influential sanction, pure as virtue, concise and precise as truth, which I found in the handwriting of our colleague. Xenocrates, historians say, who was celebrated among the Greeks for his honesty, being called to bear witness before a tribunal, the judges with common consent stopped him as he was advancing towards the altar according to the usual custom, and said, "These formalities are not required from you; an oath would add nothing to the authority of your words." Such, Bailly presents himself to the reader of his Posthumous Memoirs. None of his assertions leave any room for indecision or doubt. He needs not high-flown expressions or protestations in order to convince; nor would an oath add authority to his words. He may be deceived, but he is never the deceiver.

I will spare no effort to give to the description of the latter part of Bailly's life, all the correctness which can result from a sincere and conscientious comparison of the writings published as well by the partisans as by the enemies of our great revolution. Such, however, is my desire to prevent two phases though very distinct being confounded together, that I shall here pause, in order to cast a scrupulous glance on the actions and on the various publications of our colleague. I shall moreover thus have an easy opportunity of filling up some important lacunæ.

I read in a biographical article, otherwise very friendly, that Bailly was nominated the very day of, and immediately after, the assassination of M. de Flesselles; and in this identity the wish was to insinuate that the first Mayor of Paris received this high dignity from the bloody hands of a set of wretches. The learned biographer, notwithstanding his good will, has ill repelled the calumny. With a little more attention he would have succeeded better. A simple comparison of dates would have sufficed. The death of M.

de Flesselles occurred on the 14th of July; Bailly was nominated two days after.

I will address the same remark to the authors of a Biographical Dictionary still more recent, in which they speak of the ineffectual efforts that Bailly made to prevent the multitude from murdering the governor of the Bastille (de Launay). But Bailly had no opportunity of making an effort, for he was then at Versailles; no duty called him to Paris, nor did he become Mayor till two days after the taking of the fortress. It is really inexcusable not to have compared the two dates, by which these errors would have been avoided.

Many persons very little acquainted with contemporaneous history, fancy that during the whole duration of Bailly's administration, Paris was quite a cut-throat place. That is a romance; the following is the truth:

Bailly was Mayor during two years and four months. In that time there occurred four political assassinations; those of Foulon and of Berthier de Sauvigny, his son-in-law, at the Hôtel de Ville; that of M. Durocher, a respectable officer of the gendarmerie, killed at Chaillot, by a musket shot, in August 1789; and that of a baker massacred in a riot in the month of October of the same year. I do not speak of the assassination of two unfortunate men on the Champ-de-Mars in July 1791, as that deplorable fact must be considered separately.

The individuals guilty of the assassination of the baker were seized, condemned to death, and executed. The family of the unfortunate victim became the object of the anxious care of all the authorities, and obtained a pension.

The death of M. Durocher was attributed to some Swiss soldiers who had revolted.

The horrible and ever to be deplored assassinations of Foulon and of Berthier, are among those misfortunes which, under certain given circumstances, no human power could prevent.

In times of scarcity, a slight word, either true or unfounded, suffices to create a terrible commotion.

Réveillon is made to say, that a workman can live upon fifteen sous per diem, and behold his manufactory destroyed from top to bottom.

They ascribe to Foulon the barbarous vaunt; "I will force the people to eat hay;" and without any order from the constituted authorities, some peasants, neighbours of the old minister arrest him, take him to Paris, his son-in-law experiences the same fate, and the famished populace immolates both of them.

In proportion as the multitude appear to me unjust and culpable in attacking certain men respecting a scarcity of provisions, when

it is the manifest consequence of the severity of the seasons, I should be disposed to excuse their rage against the authors of factitious scarcities. Well, Gentlemen, at the time that Foulon was assassinated, the people, deceived by some impassioned orators of the Assembly, might, or let us rather say, ought to believe, that they were wilfully famished. Foulon perished the 22nd of July 1789; on the 15th, that is to say, seven days before, Mirabeau had addressed the following incendiary words to the inhabitants of the capital from the National Tribune:

"Henry IV. allowed provisions to be taken into besieged and rebellious Paris; but now, some perverse ministers intercept convoys of provisions destined for famished and obedient Paris."

Yet people have been so inconsiderate as to be astonished at the assassinations of Foulon and of Berthier! Going back in thought to the month of July 1789, I perceive in the imprudent apostrophe of the eloquent tribune, more sanguinary disorders than the contemporary history has had to record.

One of the most honourable, one of the most respectable and the most respected members of the institute, having been led, in a recent work, to relate the assassination of Foulon, has thrown on the conduct of Bailly, under those cruel circumstances, an aspersion that I read with surprise and grief. Foulon was detained in the Hôtel de Ville. Bailly went down into the square, and succeeded for a moment in calming the multitude. "I did not imagine," said the Mayor in his memoirs, "that they could have forced the Hôtel de Ville, a well-guarded post, and an object of respect to all the citizens. I therefore thought the prisoner in perfect safety; I did not doubt but the waves of this storm would finally subside, and I departed."

The honourable author of the *History of the Reign of Louis XVI.* opposes to this passage the following words taken from the official minutes of the Hôtel de Ville: "The electors (those who had accompanied Bailly out to the square) reported in the Hall the certainty that the calm would not last long." The new historian adds: "How could the Mayor alone labour under this delusion? It is too evident, that on such a day, the public tranquillity was much too uncertain, to allow of the chief magistrate of the town absenting himself without deserving the reproach of weakness." The remainder of the passage shows too evidently, that in the author's estimation, weakness here was synonymous with cowardice.

It is against this, Gentlemen, that I protest with heartfelt earnestness. Bailly absented himself because he did not think that the Hôtel de Ville could be forced. The electors in the passage quoted do not enunciate a different opinion: where then is the contradiction?

Bailly deceived himself in this expectation, for the multitude burst into the Hôtel de Ville. We will grant that there was an error of judgment in this; but nothing in the world authorises us to call in question the courage of the Mayor.

To decide after the blow, with so little hesitation or consideration, that Bailly ought not to have absented himself from the House of the Commune, we must forget that under such circumstances, the obligations of the first magistrate of the city were quite imperious and very numerous; it is requisite, above all, not to remember that each day, the provision of flour required for the nourishment of seven or eight hundred thousand inhabitants, depended on the measures adopted on the previous evening. M. de Crosne, who on quitting the post of Lieutenant of Police, had not ceased to be a citizen, was during some days a very enlightened and zealous councillor for Bailly; but on the day that Foulon was arrested, this dismissed magistrate thought himself lost. He and his family made an appeal to the gratitude and humanity of our colleague. It was to procure a refuge for them, that Bailly employed the few hours of absence with which he was so much reproached: those hours during which that catastrophe happened which the Mayor could not have prevented, since even the superhuman efforts of General Lafayette, commanding an armed force, proved futile. I will add, that to spare M. de Crosne an arbitrary arrest, the imminent danger of which alas! was too evident in the death of Berthier, Bailly absented himself again from the Hôtel de Ville on the night of the 22nd to the 23rd of July, to accompany the former Lieutenant of Police to a great distance from Paris.

There is not a more distressing spectacle than that of one honest man wrongfully attacking another honest man. Gentlemen, let us never willingly leave the satisfaction and the advantage of it to the wicked.

To appreciate the actions of our predecessors with impartiality and justice, it would be indispensable to keep constantly before our eyes the list of unheard-of difficulties that the revolution had to surmount, and to remember the very restricted means of repression placed at the disposal of the authorities in the beginning.

The scarcity of food gave rise to many embarrassments, to many a crisis; but causes of quite another nature had not less influence on the march of events.

In his memoirs, Bailly speaks of the manœuvres of a redoutable faction labouring for . . . . under the name of the . . . . . The names are blank. A certain editor of the work filled up the lacunæ. I have not the same hardihood. I only wished to remark that Bailly had to combat at once both the spontaneous

effervescence of the multitude, and the intrigues of a crowd of secret agents, who distributed money with a liberal hand.

Some day, said our colleague, the infernal genius who directed those intrigues and *le bailleur de fonds* will be known. Although the proper names are wanting, it is certain that some persons inimical to the revolution urged it to deplorable excesses.

These enemies had collected in the capital thirty or forty thousand vagabonds. What could be opposed to them? The Tribunals? They had no moral power, and were declared enemies to the revolution. The National Guard? It was only just formed; the officers scarcely knew each other, and moreover scarcely knew the men who were to obey them. Was it at least permitted to depend on the regular armed force? It consisted of six battalions of French Guards without officers; of six thousand soldiers who, from every part of France, had flocked singly to Paris, on reading in the newspapers the following expressions from General Lafayette: "They talk of deserters! The real deserters are those men who have not abandoned their standards." There were finally six hundred Swiss Guards in Paris, deserters from their regiments; for, let us speak freely, the celebrated monument of Lucerne will not prevent the Swiss themselves from being recognised by impartial and intelligent historians, as having experienced the revolutionary fever.

Those who, with such poor means of repression, flattered themselves that they could entirely prevent any disorder, in a town of seven or eight hundred thousand inhabitants in exasperation, must have been very blind. Those, on the other hand, who attempt to throw the responsibility of the disorders on Bailly, would prove by this alone, that good people should always keep aloof from public affairs during a revolution.

The administrator, a being of modern creation, now declares, with the most ludicrous self-sufficiency, that Bailly was not equal to the functions of a Mayor of Paris. It is, he says, by undeserved favour that his statue has been placed on the façade of the Hôtel de Ville. During his magistracy, Bailly did not create any large square in the capital, he did not open out any large streets, he elevated no splendid monument; Bailly would therefore have done better had he remained an astronomer or erudite scholar.

The enumeration of all the public erections that Bailly did not execute is correct. It might also have been added, that far from devoting the municipal funds to building, he had the vast and threatening castle of the Bastille demolished down to its very foundations; but this would not deprive Bailly of the honour of having been one of the most enlightened magistrates that the city of Paris could boast.

Bailly did not enlarge any street, did not erect any palace ɹring the twenty-eight months of his administration! No, unɔubtedly! for, first it was necessary to give bread to the inha.tants of Paris: now the revenues of the town, added to the ıily sums furnished by Necker, scarcely sufficed for those prinpal wants. Some years before, the Parisians had been very .uch displeased at the establishment of import dues on all alimenɪry substances. The writers of that epoch preserved the bur:sque Alexandrine, which was placarded all over the town, on ıe erection of the Octroi circumvallation:

"Le mur murant Paris, rend Paris murmurant."*

The multitude was not content with murmuring, the moment ıat a favourable opportunity occurred, it went to the barriers and roke them down. These were reestablished by the administration 'ith great trouble, and the smugglers often took them down by ıain force. The *Octroi* revenue from the imports, which used to mount to 70,000 francs, now fell to less than 30,000. Those ersons who have considered the figures of the present revenue, 'ill assuredly not compare such very dissimilar epochs.

But it is said that ameliorations in the moral world may often e effected without expense. What were those for which the ublic was indebted to the direct exertions of Bailly? The queson is simple, but repentance will follow the having asked it. Iy answer is this: One of the most honourable victories gained y mathematics over the avaricious prejudices of the administraons of certain towns has been, in our own times, the radical ıppression of gambling-houses. I will hasten to prove that such suppression had already engaged Bailly's attention, that he had ırtly effected it, and that no one ever spoke of those odious dens 'ith more eloquence and firmness.

"I declare," wrote the Mayor of Paris on the 5th of May 1790, that the gambling-houses are in my opinion a public scourge. think that these meetings not only should not be tolerated, but ıat they ought to be sought out and prosecuted, as much as the berty of the citizens, and the respect due to their homes, will 𝗅mit.

"I regard the tax that has been levied from such houses as a .sgraceful tribute. I do not think that it is allowable to employ revenue derived from vice and disorder, even to do good. In ›nsequence of these principles, I have never granted any permit › gambling houses; I have constantly refused them. I have ›nstantly announced that not only they would not be tolerated, ıt that they would be sought out and prosecuted."

* "The wall walling Paris, renders Paris wailing."

If I add that Bailly suppressed all spectacles of animal-fighting, at which the multitude cannot fail to acquire ferocious and sanguinary habits, I shall have a right to ask of every superficial writer, how he would justify the epithet of sterile, applied with such assurance to the administration of our virtuous colleague.

Anxious to carry out in practice that which had been largely recognised theoretically in the declaration of rights—the complete separation of religion from civil law,— Bailly presented himself before the National Assembly on the 14th of May, 1791, and demanded, in the name of the city of Paris, the abolition of an order of things which, in the then state of men's minds, gave rise to great abuses. If declarations of births, of marriages, and of deaths are now received by civil officers in a form agreeing with all religious opinions, the country is chiefly indebted for it to the intelligent firmness of Bailly.

The unfortunate beings for whom all public men should feel most solicitous, are those prisoners who are awaiting in prison the decrees of the courts of justice. Bailly took care not to neglect such a duty. At the end of 1790, the old tribunals had no moral power; they could no longer act; the new ones were not yet created. This state of affairs distracted the mind of our colleague. On the 18th of November, he expressed his grief to the National Assembly, in terms full of sensibility and kindness. I should be culpable if I left them in oblivion.

"Gentlemen, the prisons are full. The innocent are awaiting their justification, and the criminals an end to their remorse. All breathe an unwholesome air, and disease will pronounce terrible decrees. Despair dwells there: Despair says, either give me death, or judge me. When we visit those prisons, that is what the fathers of the poor and the unfortunate hear; this is what it is their duty to repeat to the fathers of their country. We must tell them that in those asylums of crime, of misery, and of every grief, time is infinite in its duration; a month is a century, a month is an abyss the sight of which is frightful ..... We ask of the tribunals to empty the prisons by the justification of the innocent, or by examples of justice."

Does it not appear to you, Gentlemen, that calm times may occasionally derive excellent lessons, and, moreover, lessons expressed in very good language, from our revolutionary epoch?

THE KING'S FLIGHT. — EVENTS ON THE CHAMP-DE-MARS.

In the month of April, 1791, Bailly perceived that his influence over the Parisian population was decreasing. The king had announced that he should depart on the 18th, and would remain some days at St. Cloud. The state of his health was the osten-

sible cause of his departure. Some religious scruples were probably the real cause; the holy week was approaching, and the king would have no communications with the ecclesiastics sworn in for his parish. Bailly was not discomposed at this projected journey; he regarded it even with satisfaction. Foreign courts, said our colleague, looked upon him as a prisoner. The sanction he gives to various decrees, appears to them extorted by violence; the visit of Louis XVI. to Saint Cloud will dissipate all these false reports. Bailly therefore concerted measures with La Fayette for the departure of the royal family: but the inhabitants of Paris, less confiding than their mayor, already saw the king escaping from St. Cloud, and seeking refuge amidst foreign armies. They therefore rushed to the Tuileries, and notwithstanding all the efforts of Bailly and his colleague, the court carriages could not advance a step. The king and queen therefore, after waiting for an hour and a-half in their carriage, reascended into the palace.

To remain in power after such a check, was giving to the country the most admirable proof of devotion.

In the night of the 20th to the 21st of June, 1791, the king quitted the Tuileries. This flight, so fatal to the monarchy, irretrievably destroyed the ascendancy that Bailly had exercised over the capital. The populace usually judges from the event. The king, they said, with the queen and their two children, were freely allowed to go out of the palace. The Mayor of Paris was their accomplice, for he has the means of knowing everything; otherwise he might be accused of carelessness, or of the most culpable negligence.

These attacks were not only echoed in the shops, in the streets, but also in the strongly organised clubs. The Mayor answered in a peremptory manner, but without entirely effacing the first impression. During several days after the king's flight, both Bailly and La Fayette were in personal danger. The National Assembly had often to look to their safety.

I have now reached a painful portion of my task, a frightful event, that led finally to Bailly's cruel death; a bloody catastrophe, the relation of which will perhaps oblige me to allow a little blame to hover over some actions of this virtuous citizen, whom thus far it has been my delight to praise without any restriction.

The flight of the king had an immense influence on the progress of our first revolution. It threw into the republican party some considerable political characters who, till then, had hoped to realise the union of a monarchy with democratical principles.

Mirabeau, a short time before his death, having heard this pro-

jected flight spoken of, said to Cabanis: "I have defended monarchy to the last; I defend it still, although I think it lost . . . . But, if the king departs, I will mount the tribune, have the throne declared vacant, and proclaim a Republic."

After the return from Varennes, the project of substituting a republican government for a monarchical government was very seriously discussed by the most moderate members of the National Assembly, and we now know that the Duke de La Rochefoucauld and Dupont (de Némours) for example, were decidedly in favour of a republic. But it was chiefly in the clubs that the idea of such a radical change had struck root. When the Commission of the National Assembly had expressed itself, through M. Muguet, at the sitting of the 13th of July, 1791, against the forfeiture of Louis XVI., there was a great fermentation in Paris. Some agents of the Cordeliers (Shoemakers') Club were the first to ask for signatures to a petition on the 14th of July, against the proposed decision. The Assembly refused to read and even to receive it. On the motion of Laclos, the club of the Jacobins got up another. This, after undergoing some important modifications, was to be signed on the 17th on the Champ de Mars, on the altar of their country. These projects were discussed openly, in full daylight. The National Assembly deemed them anarchical. On the 16th of July it called to its bar the municipality of Paris, enjoining it to have recourse to force, if requisite, to repress any culpable movements.

The Council of the Commune on the morning of the 17th placarded a proclamation that it had prepared according to the orders of the National Assembly. Some municipal officers went about preceded by a trumpeter, to read it in various public squares. Around the Hôtel de Ville, the military arrangements, commanded by La Fayette, led to the expectation of a sanguinary conflict. All at once, on the opening of the sitting of the National Assembly, a report was circulated that two good citizens having dared to tell the people collected around their country's altar, that they must obey the law, had been put to death, and that their heads, stuck upon pikes, were carried through the streets. The news of this attack excited the indignation of all the deputies, and under this impression, Alexander Lameth, then President of the Assembly, of his own accord transmitted to Bailly very severe new orders, a circumstance which, though only said *en passant*, has been but recently known.

The municipal body, as soon as it was informed, about eleven o'clock, of the two assassinations, deputed three of its members, furnished with full powers, to re-establish order. Strong detachments accompanied the municipal officers. About two o'clock it

was reported that stones had been thrown at the National Guard. The Municipal Council instantly had martial law proclaimed on the Place de Grève, and the red flag suspended from the principal window of the Hôtel de Ville. At half past five o'clock, just when the municipal body was about to start for the Champ-de-Mars, the three counsellors, who had been sent in the morning to the scene of disorder, returned, accompanied by a deputation of twelve persons, taken from among the petitioners. The explanations given on various sides occasioned a new deliberation of the Council. The first decision was maintained, and at six o'clock the municipality began its march with the red flag, three pieces of cannon, and numerous detachments of the National Guard.

Bailly, as chief of the municipality, found himself at this time in one of those solemn and perilous situations, in which a man becomes responsible in the eyes of a whole nation, in the eyes of posterity, for the inconsiderate or even culpable actions of the passionate multitude that surrounds him, but which he scarcely knows, and over which he has little or no influence.

The National Guard, in that early epoch of the revolution, was very troublesome to lead and to rule. Insubordination appeared to be the rule in its ranks; and hierarchical obedience a very rare exception. My remark may perhaps appear severe: well, Gentlemen, read the contemporary writings, Grimm's Correspondence, for example, and you will see, under date of November 1790, a dismissed captain replying to the regrets of his company in the following style: "Console yourselves, my companions, I shall not quit you; only, henceforward I shall be a simple fusilier; if you see me resolved to be no longer your chief, it is because I am content to command in my turn."

It is allowable besides to suppose that the National Guard of 1791 was deficient, in the presence of such crowds, of that patience, that clemency, of which the French troops of the line have often given such perfect examples. It was not aware that, in a large city, crowds are chiefly composed of the unemployed and the idly curious.

It was half past seven o'clock when the municipal body arrived at the Champ-de-Mars. Immediately some individuals placed on the glacis exclaimed: "Down with the red flag! down with the bayonettes!" and threw some stones. There was even a gun fired. A volley was fired in the air to frighten them; but the cries soon recommenced; again some stones were thrown; then only the fatal fusillade of the National Guard began!

These, Gentlemen, are the deplorable events of the Champ-de-Mars, faithfully analysed from the relation that Bailly himself gave of the 18th July to the Constituent Assembly. This

recital, the truth of which no one assuredly will question any more than myself, labours under some involuntary but very serious omissions. I will indicate them, when the march of events leads us, in following our unfortunate colleague, to the revolutionary tribunal.

BAILLY QUITS THE MAYORALTY THE 12TH OF NOVEMBER, 1791. — THE ESCHEVINS. — EXAMINATION OF THE REPROACHES THAT MIGHT BE ADDRESSED TO THE MAYOR.

I resume the biography of Bailly at the time when he quitted the Hôtel de Ville after a magistracy of about two years.

On the 12th November 1791, Bailly convoked the Council of the Commune, rendered an account of his administration, solemnly entreated those who thought themselves entitled to complain of him, to say so without reserve; so resolved was he to bow to any legitimate complaints; installed his successor Pétion, and retired. This separation did not lead to any of those heart-felt demonstrations from the co-labourers of the late Mayor, which are the true and the sweetest recompense to a good man.

I have sought for the hidden cause of such a constant and undisguised hostility towards the first Mayor of Paris. I asked myself first, whether the magistrate's manners had possibly excited the susceptibilities of the Eschevins.* The answer is decidedly in the negative. Bailly showed in all the relations of life a degree of patience, a suavity, a deference to the opinions of others, that would have soothed the most irascible self-love.

Must we suspect jealousy to have been at work? No, no; the persons who constituted the town-council were too obscure, unless they were mad, to attempt to vie in public consideration and glory with the illustrious author of *the History of Astronomy*, with the philosopher, the writer, the erudite scholar who belonged to our three principal academies, an honour that Fontenelle alone had enjoyed before him.

Let us say it aloud, for such is our conviction, nothing personal excited the evil proceedings, the acts of insubordination with which Bailly had daily to reproach his numerous assistants. It is even presumable, that in his position, any one else would have had to register more numerous and more serious complaints. Let us be truthful: when the *aristocracy of the ground-floor*, according to the expression of one of the most illustrious members of the French Academy, was called by the revolutionary movements to replace the *aristocracy of the first-floor*, it became giddy. Have I not, it said, conducted the business of the warehouse, the workshop, the counting-house, &c., with probity and success;

* *Eschevin* was a sort of town-council-man, peculiar to Paris and to Rotterdam, acting under a mayor.

why then should I not equally succeed in the management o public affairs? And this swarm of new statesmen were in a hurry to commence work; hence all control was irksome to them and each wished to be able to say on returning home, "I hav framed such or such an act that will tie the hands of faction fo ever; I have repressed this or that riot; I have, in short, save the country by proposing such or such a measure for the publi good, and by having it adopted." The pronoun *I* so agreeabl tickles the ear of a man lately risen from obscurity.

What the thorough-bred Eschevin, whether new or old, dread above everything else, is specialitics. He has an insurmountabl antipathy towards men, who have in the face of the world gaine the honourable titles of historian, geometer, mechanician, astro nomer, physician, chemist, or geologist, &c. . . . . . His desire, hi will, is to speak on everything. He requires, therefore, colleague who cannot contradict him.

If the town constructs an edifice, the Eschevin, losing sight o the question, talks away on the aspect of the façades. He declare with the imperturbable assurance inspired by a fact that he ha heard speak of whilst on the knees of his nurse, that on a par ticular side of the future building, the moon, an active agent o destruction, will incessantly corrode the stones of the frontage the shafts of the columns, and that it will efface in a few year all the projecting ornaments; and hence the fear of the moon' voracity will lead to the upsetting of all the views, the studies and the well-digested plans of several architects. Place a meteo rologist on the council, and, despite the authority of the nurses a whole scaffolding of gratuitous suppositions will be crumbled t dust by these few categorical and strict words of science; th moon does not exert the action that is attributed to it.

At another time, the Eschevin hurls his anathema at the systen of warming by steam. According to him, this diabolical inven tion is an incessant cause of damp to the wood-work, the fur niture, the papers, and the books. The Eschevin fancies, in short that in this way of warming, torrents of watery vapour ente into the atmosphere of the apartments. Can he love a colleague I ask, who after having had the cunning patience to let hir come to the conclusion of his discourse, informs him that, althoug vapour, the vehicle of an enormous quantity of latent heat, rapidl conveys this caloric to every floor of the largest edifice, it ha never occasion therefore to escape from those impermeable tube through which the circulation is effected!

Amidst the various labours that are required by every larg town, the Eschevin thinks, some one day, that he has discovered a infallible way of revenging himself of specialities. Guided by th

light of modern geology, it has been proposed to go with an immense sounding line in hand, to seek in the bowels of the earth the incalculable quantities of water, that from all eternity circulate there without benefiting human nature, to make them spout up to the surface, to distribute them in various directions, in large cities, until then parched, to take advantage of their high temperature, to warm economically the magnificent conservatories of the public gardens, the halls of refuge, the wards of the sick in hospitals, the cells of madmen. But according to the old geology of the Eschevin, promulgated perhaps by his nurse, there is no circulation in subterranean water; at all events, subterranean water cannot be submitted to an ascending force and rise to the surface; its temperature would not differ from that of common well-water. The Eschevin, however, agrees to the expensive works proposed. Those works, he says, will afford no material result; but once for all, such fantastic projects will receive a solemn and rough contradiction, and we shall then be liberated for ever from the odious yoke under which science wants to enslave us.

However, the subterranean water appears. It is true that a clever engineer had to bore down 548 mètres (or 600 yards) to find it; but thence it comes transparent as crystal, pure as if the product of distillation, warmed as physical laws had shown that it would be, more abundant indeed than they had dared to foresee, it shot up thirty-three mètres above the ground.

Do not suppose, Gentlemen, that putting aside wretched views of self-love, the Eschevin would applaud such a result. He shows himself, on the contrary, deeply humiliated. And he will not fail in future to oppose every undertaking that might turn out to the honour of science. Crowds of such incidents occur to the mind. Are we to infer thence, that we ought to be afraid of seeing the administration of a town given up to the stationary and exclusive spirit of the old Eschevinage — to people who have learnt nothing and studied nothing? Such is not the result of these long reflections. I wished to enable people to foresee the struggle, not the defeat. I even hasten to add, that by the side of the surly, harsh, rude, positive Eschevin, the type of whom, to say the truth, is fortunately becoming rare, an honourable class of citizens exists, who, content with a moderate fortune laboriously acquired, live retired, charm their leisure with study, and magnanimously place themselves, without any interested views, at the service of the community. Everywhere similar auxiliaries fight courageously for truth as soon as they perceive it. Bailly constantly obtained their concurrence; as is proved by some touching testimonies of gratitude and sympathy. As to the counsellors who so often occasioned trouble, confusion, and anarchy in the Hôtel de Ville in

the years '89 and '90, I am inclined to blame the virtuous magistrate for having so patiently, so diffidently endured their ridiculous pretensions, their unbearable assumption of power.

From the earliest steps in the important study of nature, it becomes evident that facts unveiled to us in the lapse of centuries, are but a very small fraction, if we compare them with those that still remain to be discovered. Placing ourselves in that point of view, deficiency in diffidence would just be the same as deficiency in judgment. But, by the side of positive diffidence, if I may be allowed the expression, relative diffidence comes in. This is often a delusion; it deceives no one, yet occasions a thousand difficulties. Bailly often confounded them. We may regret, I think, that in many instances, the learned academician disdained to throw in the face of his vain fellow-labourers these words of an ancient philosopher: "When I examine myself, I find I am but a pigmy; when I compare myself, I think I am a giant."

If I were to cover with a veil that which appeared to me susceptible of criticism in the character of Bailly, I should voluntarily weaken the praises that I have bestowed on several acts of his administration. I will not commit this fault, no more than I have done already in alluding to the communications of the mayor with the presuming Eschevins.

I will therefore acknowledge that on several occasions, Bailly, in my opinion, showed himself influenced by a petty susceptibility, if not about his personal prerogatives, yet about those of his station.

I think also that Bailly might be accused of an occasional want of foresight.

Imaginative and sensitive, the philosopher allowed his thoughts to centre too exclusively on the difficulties of the moment. He persuaded himself, from an excess of good will, that no new storm would follow the one that he had just overcome. After every success, whether great or small, against the intrigues of the court, or prejudices, or anarchy, whether President of the National Assembly or Mayor of Paris, our colleague thought the country saved. Then his joy overflowed; he would have wished to spread it over all the world. It was thus that on the day of the definite re-union of the nobility with the other two orders, the 27th of June 1789, Bailly going from Versailles to Chaillot, after the close of the session, leaned half his body out of his carriage door, and announced the happy tidings with loud exclamations to all whom he met on the road. At Sèvres, it is from himself that I borrow the anecdote, he did not see without painful surprise that his communication was received with the most complete indifference by a group of soldiers assembled before the barrack door: Bailly

laughed much on afterwards learning that this was a party of Swiss soldiers, who did not understand a word he said.

Happy the actors in a great revolution, in whose conduct we find nothing to reprehend until after having entered into so minute an analysis of their public and private conduct.

BAILLY'S JOURNEY FROM PARIS TO NANTES, AND THEN FROM NANTES TO MELUN.—HIS ARREST IN THIS LAST TOWN.—HE IS TRANSFERRED TO PARIS.

After having quitted the Mayoralty of Paris, Bailly retired to Chaillot, where he hoped again to find happiness in study; but upwards of two years passed amidst the storms of public life had deeply injured his health; it was therefore requisite to obey the advice of physicians, and undertake a journey. About the middle of June 1792, Bailly quitted the capital, made some excursions in the neighbouring departments, went to Niort to visit his old colleague and friend, M. de Lapparent, and soon after went on far as Nantes, where the due influence of another friend, M. Gelée de Prémion, seemed to promise him protection and tranquillity. Determined to establish himself in this last town, Bailly and his wife took a small lodging in the house of some distinguished people, who could understand and appreciate them. They hoped to live there in peace; but news from Paris soon dissipated this illusion. The Council of the Commune decreed, that the house previously occupied, in consequence of a formal decision, by the Mayor of Paris, and by the public offices of the town, ought to have paid a tax of 6,000 livres, and, strange enough, that Bailly was responsible for it. The pretended debt was claimed with harshness. They demanded the payment of it without delay, To free himself Bailly was obliged to sell his library, to abandon to the chances of an auction that multitude of valuable books, from which he had sought out, in the silence of his study, and with such remarkable perseverance, the most recondite secrets of the firmament.

This painful separation was followed by two acts that did not afflict him less.

The central government (then directed, it must be allowed, by the Gironde party,) placed Bailly under surveillance. Every eight days the venerable academician was obliged to present himself at the house of the Syndic Procurator of the Departmental Administration of the Lower-Loire, like a vile malefactor, whose every footstep it would be to the interest of society to watch. What was the true motive for such a strange measure? This secret has been buried in a tomb where I shall not allow myself to dig for it.

Though painful to me to say so, the odious assimilation of Bailly to a dangerous criminal had not exhausted the rancour of his enemies. A letter from Roland, the Minister of the Interior, announced very drily to the unfortunate proscribed man, that the apartments in the Louvre, which his family had occupied for upwards of half a century, had been withdrawn from him. They had even proceeded so far as to furnish a tipstaff with the order to clear the rooms.

A short time before this epoch, Bailly had found himself obliged to sell his house at Chaillot. The old Mayor of Paris then had no longer a hearth or a home in the great city which had been the late scene of his devotion, his solicitude, and his sacrifices. When this reflection occurred to his mind, his eyes filled with tears.

But the grief that Bailly experienced on seeing himself the daily object of odious persecutions, left his patriotic convictions intact. Vainly did they endeavour several times to transform a legitimate hatred towards individuals into an antipathy towards principles. They still remember in Brittany the debate raised, by one of these attempts, between our colleague and a Vendéan physician, Dr. Blin. Never, in the season of his greatest popularity, did the president of the National Assembly express himself with more vivacity; never had he defended our first revolution with more eloquence. Not long since, in this same place, I pointed out to public attention another of our colleagues (Condorcet), who already under the blow of a capital condemnation, devoted his last moments to restore to the light of day the principles of eternal justice, which the passions and the follies of men had but too much obscured. At a time of weak or interested convictions, and disgraceful capitulations of conscience, those two examples of unchangeable convictions deserved to be remarked. I am happy in having found them in the bosom of the Academy of Sciences.

Tranquillity of mind is not less requisite than vigour of intellect, to those who undertake great works. Thus during his residence at Nantes, Bailly did not even try to add to his numerous scientific or literary productions. This celebrated astronomer passed his time in reading novels. He sometimes said with a bitter smile: "My day has been well occupied: since I got up, I have put myself in a position to give an analysis of the two, or of the three first volumes of the new novel that the reading-room has just received." From time to time these abstractions were of a more elevated tone; he owed them to two young persons, who having reached an advanced age may now be listening to my words. Bailly discoursed with them of Homer, of Plato, of Aristotle, of

the principal works in our literature, of the rapid progress of the sciences, and chiefly of those of astronomy. What our colleague chiefly appreciated in these two young friends, was a true sensibility, and great warmth of feeling. I know that years have not effaced or weakened these rare qualities in the bosoms of those two Brétons. M. Pariset, our colleague, and M. Villenave, will therefore think it natural in me to thank them here, in the name of science and literature, in the name of humanity, for the few moments of sweet peace and happiness that they afforded to our learned colleague, at a time when the inconstancy and ingratitude of men were lacerating his heart.

Louis XVI. had perished; dark clouds hung over the horizon; some acts of odious brutality showed our proscribed philosopher how little he must thenceforward depend on public sympathy; how much times had changed since the memorable meeting (of the 7th of October 1791), at which the National Assembly decided that the bust of Bailly should be placed in the hall of their meetings! The storm appeared near and very menacing; even persons usually of little foresight were meditating where to find shelter.

During these transactions, Charles Marquis de Casaux, known by various productions on literature and on economical politics, went and requested our colleague, together with his wife, to take a passage on board a ship that he had freighted for himself and his family. "We will first go to England," said M. Casaux; "we will then, if you prefer it, pass our exile in America. Have no anxiety, I have property; I can, without inconvenience to myself, undertake all the expenses. Pythagoras said: 'In solitude the wise man worships echo;' but this no longer suffices in France: the wise man must fly from a land that threatens to devour its children."

These warm solicitations, and the prayers of his weeping companion could not shake the firm resolution of Bailly. "From the day that I became a public character," he said, "my fate has become irrevocably united with that of France; never will I quit my post in the moment of danger. Under any circumstances my country may depend on my devotion. Whatever may happen, I shall remain."

By regulating his conduct on such fine generous maxims, a citizen does himself honour, but he exposes himself to fall under the blows of faction.

Bailly was still at Nantes on the 30th of June 1793, when eighty thousand Vendéans, commanded by Cathelineau and Charette, went to besiege that city.

Let us imagine to ourselves the position of the President of the

sitting of the "Jeu de Paume," of the first Mayor of Paris, in a city besieged by the Vendéans! We cannot presume that the unfavourable opinion of the Convention under which he was labouring, and the rigorous surveillance to which he was subjected, would have saved him from harsh treatment if the town had been taken. No one can therefore be surprised that after the victory of the Nanteans, our colleague hastened to follow out his project, formed a short time before, of withdrawing from the insurgent provinces.

Up to the beginning of July 1793, Melun had enjoyed perfect tranquillity. Bailly knew it through M. de Laplace, who, living retired in that chief town of the department was there composing the immortal work, in which the wonders of the heavens are studied with so much depth and genius. He also knew that the great geometer, hoping to be still more retired in a cottage on the banks of the Seine, and out of the town, was going to dispose of his house in Mélun. It is easy to guess that Bailly would be charmed with the prospect of residing far away from political agitation, and near to his illustrious friend!

The arrangements were promptly made, and on the 6th of July, M. and Madame Bailly quitted Nantes in company with M. and Madame Villenave, who were going to Rennes.

At this same time, a division of the revolutionary army was marching to Mélun. As soon as the terrible news was known, Madame Laplace wrote to Bailly, persuading him, under covert expressions, to give up the intended project. The house, she said, is at the water's edge: there is extreme dampness in the rooms: Madame Bailly would die there. A letter so different from those that had preceded it, could not fail of its effect; such at least was the hope with which M. and Madame Laplace flattered themselves, when about the end of July they perceived, with inexpressible alarm, Bailly crossing the garden path. "Great God, you did not then understand our last letter!" exclaimed at the same instant our colleague's two friends. "I understood perfectly," Bailly replied with the greatest calm; "but on the one hand, the two servants who followed me to Nantes, having heard that I was going to be imprisoned, quitted me; on the other hand, if I am to be arrested, I wish it to be in a house that I have occupied some time. I will not be described in any act as an individual without a domicile!" Can it be said, after this, that great men are not subject to strange weaknesses?

These minute details will be my only answer to some culpable expressions that I have met with in a work very widely spread: "M. Laplace," says the anonymous writer, "knew all the secrets of geometry; but he had not the least notion of the state France

was in, he therefore imprudently advised Bailly to go and join him."

What is to be here deplored as regards imprudence, is, that a writer, without exactly knowing the facts, should authoritatively pronounce such severe sentences against one of the most illustrious ornaments of our country.

Bailly did not even enjoy the puerile satisfaction of taking rank among the domiciled citizens of Mélun. For two days after his arrival in that town, a soldier of the revolutionary army having recognised him, brutally ordered him to accompany him to the municipality: "I am going there," coolly replied Bailly; "you may follow me there."

The municipal body of Mélun had at that time an honest and very courageous man at its head, M. Tarbé des Sablons. This virtuous magistrate endeavoured to prove to the multitude, (with which the Hôtel de Ville was immediately filled by the news, rapidly propagated, of the arrest of the old Mayor of Paris,) that the passports granted at Nantes, countersigned at Rennes, showed nothing irregular; that according to the terms of the law, he could not but set Bailly at liberty, under pain of forfeiture. Vain efforts! To avoid a bloody catastrophe, it was necessary to promise that reference would be made to Paris, and that in the meantime he should be guarded— *à vue* — in his own house.

The surveillance, perhaps purposely, was not at all strict; to escape would have been very easy. Bailly utterly discarded the notion. He would not at any price have compromised M. Tarbé, nor even his guard.

An order from the Committee of Public Safety enjoined the authorities of Mélun to transfer Bailly to one of the prisons of the capital. On the day of departure, Madame Laplace paid a visit to our unfortunate colleague. She represented to him again the possibility of escape. The first scruples no longer existed; the escort was already waiting in the street. But Bailly was inflexible. He felt perfectly safe. Madame Laplace held her son in her arms; Bailly took the opportunity of turning the conversation to the education of children. He treated the subject, to which he might well have been thought a stranger, with a remarkable superiority, and ended even with several amusing anecdotes that would deserve a place in the witty and comic gallery of "les Enfants terribles."

On arriving at Paris, Bailly was imprisoned at the Madelonnettes, and some days after at La Force. They there granted him a room, where his wife and his nephews were permitted to visit him.

Bailly had undergone only one examination of little importance, when he was summoned as a witness in the trial of the queen.

BAILLY IS CALLED AS A WITNESS IN THE TRIAL OF THE QUEEN. — HIS OWN TRIAL BEFORE THE REVOLUTIONARY TRIBUNAL. — HIS CONDEMNATION TO DEATH. — HIS EXECUTION. — IMAGINARY DETAILS ADDED BY ILL-INFORMED HISTORIANS TO WHAT THAT ODIOUS AND FRIGHTFUL EVENT ALREADY PRESENTED.

Bailly, under the weight of a capital accusation, and precisely on account of a portion of the acts imputed to Marie Antoinette, was heard as a witness in the trial of that princess. The annals of tribunals, either ancient or modern, never offered anything like this. What did they hope for? To lead our colleague to make inexact declarations, or to concealments from a feeling of imminent personal danger? To suggest the thought to him to save his own head at the expense of that of an unhappy woman? To make virtue finally stagger? At all events, this infernal combination failed; with a man like Bailly it could not succeed.

"Do you know the accused?" said the President to Bailly. "Oh! yes, I do know her!" answered the witness, in a tone of emotion, and bowing respectfully to Marie Antoinette. Bailly then protested with horror against the odious imputations that the act of accusation had put into the mouth of the young dauphin. From that moment Bailly was treated with great harshness. He seemed to have lost in the eyes of the tribunal the character of a witness, and to have become the accused. The turn that the debates took would really authorise us to call the sitting in which the queen was condemned, (in which she figured ostensibly as the only one accused) the trial of Marie Antoinette and of Bailly. What signified, after all, this or that qualification of this monstrous trial? in the judgment of any man of feeling, never did Bailly prove himself more noble, more courageous, more worthy, than in this difficult situation.

Bailly appeared again before the Revolutionary Tribunal, and this time as the accused, the 10th of November 1793. The accusation bore chiefly on the pretended participation of the Mayor of Paris in the escape of Louis XVI. and his family, and in the catastrophe that occurred in the Champ de Mars.

If any thing in the world appeared evident, even in 1793, even before the detailed revelations of the persons who took a more or less direct part in the event, it is, that Bailly did not facilitate the departure of the royal family; it is that, in proportion to the suspicions that reached him, he did all that was in his power to prevent their departure; it is, that the President of the sitting of the Jeu de Paume had not, and could never have had in any case, an intention of going to join the fugitive family in a strange country; it is that, finally, any act emanating from a public authority in which such expressions as the following could be found: "The deep

wickedness of Bailly . . . . . Bailly thirsted for the people's blood!" must have excited the disgust and indignation of good men, whatever might be their political opinions.

The accusation, as far as it regarded the murderous fusillade on the Champ de Mars, had more weight; this event had as counterpoises, the 10th of August and the 31st of May; La Fayette says in his memoirs, that those two days were a retaliation. It is at least certain that the terrible scenes of the 17th of July cost Bailly his life; they left deep impressions in people's minds, which were still perceptible after the revolution of 1830, and which, on more than one occasion, rendered the position of La Fayette one of great delicacy. I have therefore studied them most attentively, with a very sincere and lively desire to dissipate, once for all, the clouds that seemed to have obscured this point, this sole point, in the life of Bailly. I have succeeded, Gentlemen, without ever having had a wish or occasion to veil the truth. I do no Frenchman the injustice to suppose that I need define to him an event of the national history that has been so influential on the progress of our revolution, but perhaps, there may be some foreigners present at this sitting. It will be therefore for them only that I shall here relate some details. We must bring to mind some deplorable circumstances of the evening of the 17th July, when the multitude had assembled on the Champ de Mars or Champ de la Fédération, around the altar of their country, the remains of the wooden edifice that had been raised to celebrate the anniversary of the 14th of July. Part of this crowd signed a petition tending to ask the forfeiture of the throne by Louis XVI., then lately reconducted from Varennes, and on whose fate the Constituent Assembly had been enacting regulations. On that occasion martial law was proclaimed. The National Guard, with Bailly and La Fayette at their head, went to the Champ de Mars; they were assailed by clamours, by stones, and by the firing of a pistol; the Guard fired; many victims fell, without its being possible to say exactly how many, for the estimates, according to the effect that the reporters wished to produce, varied from eighty to two thousand!

The Revolutionary Tribunal heard several witnesses relative to the events on the Champ de Mars: amongst them I find Chaumette, Procurator of the Commune of Paris; Lullier, the Syndic Procurator General of the Department; Coffinhal, Judge of the Revolutionary Tribunal; Dufourny, manufacturer of gunpowder; Momoro, a printer.

All these witnesses strongly blamed the old Mayor of Paris; but who is there that does not know how much arbitrariness and cruelty these individuals, whom I have mentioned above, showed

during our misfortunes? Their declarations, therefore, must be received with great suspicion.

The sincere admirers of Bailly would be relieved of a great weight, if the event of the Champ de la Fédération had been darkened only by the testimonies of Chaumettes and Coffinhals. Unfortunately, the public accuser produced some very grave documents during the debates, which the impartial historian cannot overlook. Let us say, however, just to correct one error out of a thousand, that on the day of Bailly's trial, the public accuser was Naulin, and not Fouquier Tinville, notwithstanding all that has been written on this subject by persons calling themselves well-informed, and even some of the accused's intimate friends.

The catastrophe of the Champ de Mars, when impartially examined in its essential phases, presents some very simple problems:

Was a petition to the Constituent Assembly illegal that was got up on the 17th of July, 1791, against a decree issued on the 15th?

Had the petitioners, by assembling on the Champ de Mars, violated any law?

Could the two murders committed in the morning be imputed to these men?

Had projects of disorder and rebellion been manifested with sufficient evidence to justify the proclamation of martial law, and especially the putting it into practice?

I say it, Gentlemen, with deep grief, these problems will be answered in the negative by whoever takes the trouble to analyse without passion, and without preconceived opinions, some authentic documents, which people in general seem to have made it a point to leave in oblivion. But I hasten to add, that considering the question as to intention, Bailly will continue to appear, after this examination, quite as humane, quite as honourable, quite as pure as we have found him to be in the other phases of a public and private life, which might serve as a model.

In the best epochs of the National Assembly, no one who belonged to it would have dared to maintain, that to draw up and sign a petition, whatever might be the object of it, were rebellious acts. Never, at that time, would the President of that great Assembly have called down hate, public vengeance, or a sanguinary repression upon those who attempted, said Charles Lameth, in the sitting of the 16th of July, "to oppose their individual will to the law, which is an expression of the national will." The right of petition seemed as if it ought to be absolute, even if contrary to sanctioned and promulgated laws in full action, and even more so against legislative arrangements still under discussion, or scarcely voted.

The petitioners of the Champ de Mars asked the Constituent Assembly to revise a decree that they had issued two days before. We have no occasion to examine whether the act was reasonable, opportune, dictated by an enlightened view of the public good. The question is simple; in soliciting the Assembly to revise a decree, they violated no law. Perhaps it will be thought that the petitioners at least committed an unusual act, contrary to all custom. Even this would be unfounded. In ten various instances, the National Assembly modified or annulled its own decrees; in twenty others, it had been entreated to revise them, without any cry of anarchy being raised.

It is well ascertained, that the crowd on the Champ de Mars availed itself of a right that the constitution recognised, that of getting up and signing a petition against a decree which, right or wrong, it thought was opposed to the true interests of the country. Still the exercise of the right of petitioning was always wisely subjected to certain forms. Had these forms been violated? Was the meeting illegal?

In 1791, according to the decrees, every meeting that wished to exercise the right of petition must consist of unarmed citizens, and be announced to the competent authorities twenty-four hours beforehand.

Well, on the 16th of July, twelve persons had gone as a deputation to the municipality, in order to declare, according to law, that the next day, the 17th, numerous citizens would meet, without arms, on the Champ de Mars, where they wished to sign a petition. The deputation obtained an acknowledgment of its declaration from the hand of the syndic procurator Desmousseaux, who addressed them besides with these solemn words: "The law shields you with its inviolability."

The acknowledgment was presented to Bailly on the day of his condemnation.

Had they committed some assassinations? Yes, undoubtedly; they had committed two; but in the morning, very early; but at the Gros Caillou, and not on the Champ de Mars. Those horrid murders could not legitimately be imputed to the petitioners who, eight or ten hours after, surrounded the altar of their country; to the crowd who fell by the fusillade of the National Guard. By changing the date of these crimes, and displacing also the localities where these crimes were committed, some historians of our revolution, and amongst others the best known of all, have given, without intending it, to the meeting in the afternoon, a character that cannot be honestly concurred in.

It is requisite we should know at what hour, in what place, and how, these misfortunes happened, before we hazard an opinion on the sanguinary acts of that day, the 17th of July.

A young man had gone that day very early to the altar of hi country. This young man wished to copy several inscriptions All at once he heard a singular noise, and very soon after th worm of a wimble shot up from the planked floor on which h was standing. The youth went and sought the guard, who raise the plank, and found beneath the altar two ill-looking individuals lying down and furnished with provisions. One of these me was an invalid with a wooden leg. The guard seized them, an took them to the Gros Caillou, to the section, to the Commissary of Police. On the way, the barrel of water with whic these unfortunate men had provided themselves under the alta of their country, was transformed, according to the ordinar course of things, into a barrel of gunpowder. The inhabitant of that quarter of the town collected together; it was on Sunday. The women especially showed themselves very muc irritated when the purpose of the auger-holes was told them, a declared by the invalid. When the two prisoners came out o the hall to be conducted to the Hôtel-de-Ville, the crowd tore the from the guard, massacred them, and paraded their heads o pikes!

It cannot be too often repeated, that these hideous assassinations this execution of two old vagabonds by the barbarous and blinde population of the Gros Caillou, evidently had no relation to, n connection with, the events which, in the evening, carried mourning into the Champ de la Fédération.

On the evening of the 17th of July, from five to seven o'clock had the crowd which was collected around the altar of thei country an aspect of turbulence, giving reason to fear a riot, sedition, violence, or any anarchical enterprise?

Relative to this point, we have the written declaration of thre councillors, whom the municipality had sent in the morning t the Gros Caillou, on the first intimation of the two assassination of which I have just spoken. This declaration was presented t Bailly on the day of his condemnation. We read therein, "tha the assembled citizens on the Champ de Mars had in no wa acted contrary to law; that they only asked for time to sign thei petition before they retired; that the crowd had shown all possible respect to the commissaries, and given proofs of submissio to the law and its agents." The Municipal Councillors, on thei return to the Hôtel-de-Ville, accompanied by a deputation of twelv of the petitioners, protested strongly against the proclamation o martial law; they declared that if the red flag was unfurled, the would be regarded, and with some appearance of reason, as traitor and faithless men.

Vain efforts; the anger of the councillors, confined since th

morning at the Hôtel-de-Ville, carried the day over the enlightened opinion of those who had been sent scrupulously to study the state of affairs, who had mixed in the crowd, who returned after having reassured it by promises.

I might invoke the testimony of one of my honourable colleagues. Led by the fine weather, and somewhat also by curiosity, towards the Champ de Mars, he was enabled to observe all; and he has assured me that there never was a meeting which showed less turbulence or seditious spirit; that especially the women and children were very numerous. Is it not, besides, perfectly proved now, that on the morning of the 17th July, the Jacobin club, by means of printed placards, disavowed any intention of petitioning; and that the influential men of the Jacobins and of the Cordeliers,—those men whose presence might have given to this concourse the dangerous character of a riot,—not only did not appear there, but had started in the night for the country?

By thus connecting together all the circumstances whence it is proved that martial law was proclaimed and put in practice on the 17th of July without legitimate motives, a most terrible responsibility seems at first sight to be cast on the memory of Bailly. But reassure yourselves, Gentlemen; the events which are now grouped together, and are exhibited to our eyes with complete evidence, were not known on that inauspicious day at the Hôtel-de-Ville, until they had been distorted by the spirit of party.

In the month of July, 1791, after the king had returned from Varennes, the monarchy and the republic began for the first time to be dangerously opposed to each other; in an instant passion took the place of cool reason in the minds of the respective partisans of the two different forms of government. The terrible formula: *We must make an end of it!* was in everybody's mouth.

Bailly was surrounded by those passionate politicians who, without the least scruple as to the honesty or legality of the means, are determined to make an end of the adversaries who annoy them, as soon as circumstances seem to promise them victory.

Bailly had still near him some Eschevins long accustomed to regard him as a magistrate for show.

The former gave the Mayor false, or highly coloured intelligence. The others, by long habit, did not conceive themselves obliged to communicate anything to him.

On the bloody day of July, 1791, of all the inhabitants of Paris, perhaps Bailly was the man who knew with least detail or correctness the events of the morning and of the evening.

Bailly, with his deep horror for falsehood, would have thought

that he was most cruelly insulting the magistrates, if he had not attributed to them similar sentiments to his own. His uprightness prevented his being sufficiently on the watch against the machinations of parties. It was evidently by false reports that he was induced to unfurl the red flag on the 17th of July: "It was from the reports that followed each other," he said to the Revolutionary Tribunal, on being questioned by the President, "and became more and more alarming every hour, that the council adopted the measure of marching with the armed force to the Champ de Mars."

In all his answers Bailly insisted on the repeated orders he had received from the President of the National Assembly; on the reproaches addressed to him for not sufficiently watching the agents of foreign powers; it was against these pretended agents and their creatures, that the Mayor of Paris thought he was marching when he put himself at the head of a column of National Guards.

Bailly did not even know the cause of the meeting; he had not been informed that the crowd wished to sign a petition; and that the previous evening, according to the decree of the law, there had been a declaration made to this effect before the competent authority. His answers to the Revolutionary Tribunal leave not the least doubt on this point!

Oh Eschevins, Eschevins! when your vain pretensions only were treated of, the public could forgive you; but the 17th of July, you took advantage of Bailly's confidence; you induced him to take sanguinary measures of repression, after having fascinated him with false reports; you committed a real crime. If it was the duty of the Revolutionary Tribunal, of deplorable memory, to demand in 1793 from any one an explanation of the massacres of the Champ de Mars, it was not Bailly assuredly who ought to have been accused in the first place.

The political party whose blood flowed on the 17th of July, pretended to have been the victim of a plot concocted by its adversaries. When interrogated by the President of the Revolutionary Tribunal, Bailly answered: "I had no knowledge of it, but experience has since given me reason to think that such a plot did exist at that time."

Nothing more serious has ever been written against the promoters of the sanguinary violences on the 17th of July.

The blame that has been thrown on the events of the Champ de Mars has not been confined solely to the fact of proclaiming martial law; the repressive measures that followed that proclamation have been criticised with equal bitterness.

The municipal administration was especially reproached for

having hoisted a red flag much too small; a flag that was called in the Tribunal *a pocket flag;* for not having placed this flag at the head of the column, as the law commands, but in such a position, that the public on whom the column was advancing could not see it; for having made the armed force enter the Champ de Mars, by all the gates on the side towards the town, a manœuvre that seemed rather intended to surround the multitude, than to disperse it; for having ordered the National Guard to load their arms, even on the Place de Grève; for having made the guard fire before the three required summonses were made, and fire upon the people around the altar, whilst the stones and the pistol shot, which were assigned as the motive for the sanguinary order, came from the steps and benches; for allowing some people who were endeavouring to escape on the side towards l'Ecole Militaire, and others who had actually jumped into the Seine, to be pursued, shot, and bayonetted.

It results clearly from one of Bailly's publications, from his answers to the questions put to him by the President of the Revolutionary Tribunal, from the writings of the day:

That the Mayor of Paris gave no order for the troops to be collected on the 17th of July; that he had had no conference on that day with the military authority; that if any arrangements, culpable and contrary to law were adopted, as to the situation of the cavalry, of the red flag, and of the Municipal Body, in the column marching on the Champ de Mars, they could not without injustice be imputed to him; that Bailly was not aware of the National Guard having loaded their muskets with ball before quitting the square of the Hôtel-de-Ville; that he was not aware even of the existence of the red flag, with whose small dimensions he had been so severely reproached; that the National Guard fired without his order; that he made every effort to stop the firing, to stop the pursuit, and make the soldiers resume their ranks; that he congratulated the troops of the line, who under the command of Hulin, entered by the gate of l'Ecole Militaire, and not only did not fire, but tore many of the unfortunate people from the hands of the National Guard, whose exasperation amounted to delirium. In short, it might be asked, relative to any want of exactness attributable to Bailly in that unfortunate affair, whether it was just to impute it to him who, in his letters to Voltaire on the origin of the sciences wrote as follows in 1776:

"I am unfortunately short-sighted. I am often humiliated in the open country. Whilst I with difficulty can distinguish a house at the distance of a hundred paces, my friends relate to me what they see at the distance of five or six hundred. I open my eyes, I fatigue myself without seeing anything, and I am sometimes inclined to think that they amuse themselves at my expense."

You begin to see, Gentlemen, the advantage that a firm and able lawyer might have drawn from the authentic facts that I have just been relating. But Bailly knew the pretended jury before whom he had to appear. This jury was not a collection of drunken cobblers, whatever some passionate writers may have asserted; it was worse than that, Gentlemen, notwithstanding the deservedly celebrated names that were occasionally interspersed among them: it was—let us cut the subject short—an odious commission.

The very circumscribed list from which chance in 1793 and 1794 drew the juries of the Revolutionary Tribunals, did not embrace, as the sacred word *jury* seems to imply, all one class of citizens. The authorities formed it, after a prefatory and very minute inquiry, of their adherents only. The unfortunate defendants were thus judged not by impartial persons free from any preconceived system, but by political enemies, which is as much as to say, by that which is the most cruel and remorseless in the world.

Bailly would not be defended. After his appearance as a witness in the trial of Marie Antoinette, the ex-Mayor only wrote and had printed for circulation, a paper entitled *Bailly to his fellow-citizens.* It closes with these affecting words:

"I have only gained by the Revolution that which my fellow-citizens have gained: liberty and equality. I have lost by it some useful situations, and my fortune is nearly destroyed. I could be happy with what remains of it to me and a clear conscience; but to be happy in the repose of my retreat, I require, my dear fellow-citizens, your esteem: I know well that, sooner or later, you will do me justice; but I require it while I live, and while I am yet amongst you."

Our colleague was unanimously condemned. We should despair of the future, unless such a unanimity struck all friends of justice and humanity with stupor, if it did not increase the number of decided adversaries to all political tribunals.

When the President of the Tribunal interrogated the accused, already declared guilty, as to whether he had any reclamations to make relative to the execution of the sentence, Bailly answered:

"I have always carried out the law, I shall know how to submit myself to it, since you are its organ."

The illustrious convict was led back to his cell.

Bailly had said in his éloge on M. de Tressan: "French gaiety produces the same effect as stoicism." These words occurred to my memory at the time when I was gathering from various sources the proof that on re-entering the Conciergerie after his condemnation, Bailly showed himself at once both gay and stoical.

He desired his nephew, M. Batbéda, to play a game at piquet with him as usual. He thought of all the circumstances connected with the frightful morrow with such coolness, that he even said with a smile to M. Batbéda during the game: "Let us rest awhile, my friend, and take a pinch of snuff; to-morrow I shall be deprived of this pleasure, for I shall have my hands tied behind my back."

I will quote some words which, while testifying to a similar degree Bailly's serenity of mind, are more in harmony with his grave character, and more worthy of being preserved in history.

One of the companions of the illustrious academician's captivity, on the evening of the 11th of November, with tears in his eyes and moved by a tender veneration, exclaimed: "Why did you let us fancy there was a possibility of acquittal? You deceived us then?"—Bailly answered: "No, I was teaching you never to despair of the laws of your country."

In the paroxysms of wild despair, some of the prisoners reviewing the past, went so far as to regret that they had never infringed the laws of the strictest honesty.

Bailly brought back these minds, erring for the moment from the path of duty, by repeating to them maxims which both in form and substance would not disparage the collections of the most celebrated moralists:

"It is false, very false, that a crime can ever be useful. The trade of an honest man is the safest, even in times of revolution. Enlightened egotism suffices to put any intelligent individual into the path of justice and truth. Whenever innocence can be sacrificed with impunity, crime is not sure of succeeding. There is so great a difference between the death of a good man and that of a wicked man, that the multitude is incapable of estimating it."

Cannibals devouring their vanquished enemies seem to me less hideous, less contrary to nature, than those wretches, the refuse of the population of large towns, who, too often alas! have carried their ferocity so far, as to disturb by their clamorous and infamous raillery the last moments of the unhappy victims about to be struck by the sword of the law. The more humiliating this picture of the degradation of the human species may be, the more we should beware of over-charging the colouring. With few exceptions, the historians of Bailly's last agony appear to me to have forgotten this duty. Was the truth, the strict truth, not sufficiently distressing? Was it requisite, without any sort of proof, to impute to the mass of the people the infernal cynicism of cannibals? Should they lightly make just sentiments of disgust and indignation rest upon an immense class of citizens? I think not, Gentlemen, and I will therefore avoid the cruelty

and poignancy of chaining the thoughts for a long time on such scenes; I will prove that by rendering the drama a little less atrocious, I have only sacrificed imaginary details, which are the envenomed fruits of the spirit of party.

I will not shut my ears to the questions that already hum around me. People will say to me, What are your claims for daring to modify a page of our revolutionary history, on which every one seemed agreed? What right have you to weaken contemporary testimonies, you, who at the time of Bailly's death, were scarcely born; you who lived in an obscure valley of the Pyrenees, two hundred and twenty leagues from the capital?

These questions do not embarrass me at all. In short, I do not ask that the relation of what seems to me to be the expression of the truth, should be adopted upon my word. I enumerate my proofs, I express my doubts. Within these limits there is no one but has claims to bring forward; the discussion is open to all the world, the public will pronounce its definitive judgment.

As a general thesis, I will add that by concentrating our researches on one circumscribed and special object, we have a better chance of seeing it correctly and knowing it well, all other things being equal, than by scattering our attention in all directions.

As to the merit of contemporaneous narratives, it seems to me very dubious. Political passions do not allow us to see objects in their real dimensions, nor in their true forms, nor in their natural colours. Moreover, have not unpublished and very valuable documents come to shed bright colours, just where the spirit of party had spread a thick veil?

The account that Riouffe gave of the death of Bailly has almost blindly led all the historians of our revolution. What does it consist of "at bottom." The prisoner of la Conciergerie said it himself; of tales related by executioners' valets, repeated by turnkeys.

I would willingly allow this account to be set against me, notwithstanding the horrid sewer from which Riouffe had been obliged to draw, if it were not evident that this clever writer saw all the revolutionary events through the just anger that an ardent and active young man must feel after an iniquitous imprisonment; if this current of sentiments and ideas had not led him into some manifest errors.

Who has not, for example, read with tears in their eyes, in the *Mémoires sur les Prisons*, what the author relates of the fourteen girls of Verdun? "Of those girls," he said, "of unparalleled fairness, and who appeared like young virgins dressed for a public fête. They disappeared," added Riouffe, "all at once, and were mowed down in the spring of life. The court occupied by the women

the day after their death, had the appearance of a garden that had been despoiled of its flowers by a storm. I have never seen amongst us a despair equal to that excited by this barbarity."

Far be from me the intention to weaken the painful feelings which the catastrophe related by Riouffe must naturally inspire; but every one has remarked that the report of this writer is very circumstantial; the author appears to have seen all with his own eyes. Yet he has been guilty of the gravest inaccuracy.

Out of the fourteen unfortunate women who were sentenced after Verdun was retaken from the Prussians, two girls of seventeen years of age were not condemned to death on account of their youth.

This first circumstance was well worth recording. Let us go farther. A historian having lately consulted the official journals of that epoch, and the bulletin of the Revolutionary Tribunal, discovered with some surprise that among the twelve *young girls* who were condemned, there were seven either married or widows, whose ages varied from forty-one to sixty-nine!

Contemporary accounts then, even those of Riouffe, may be submitted without irreverence to earnest discussion. When a tenth part of the funds annually devoted to researches in and examination of old chronicles, is applied to making extracts from the registers relative to the French Revolution, we shall certainly see many other hideous circumstances that revolt the soul, disappear from our contemporary history. Look at the massacres of September! The historians most in vogue report the number of victims that fell in that butchery to have been from six to twelve thousand; whilst a writer who has lately taken the trouble to analyse the prison registers in the gaoler's books, cannot make the whole amount to one thousand. Even this number is very large; but, for my part, I thank the author of this recent publication for having reduced the number of assassinations in September to less than a tenth part of what had been generally admitted.

When the discussion which I have here undertaken becomes known to the public, it will be seen how many and how important are the retrenchments to be made from that lugubrious page of our history. Another important circumstance may be appreciated, which appears to me to arise from all these facts. After having weighed my proofs, every one I hope will join me in seeing that the wretches around the scaffold of Bailly were but the refuse of the population, fulfilling for pay the part that had been assigned them by three or four wealthy cannibals.

The sentence pronounced against Bailly by the Revolutionary Tribunal was to be executed on the 12th of November, 1793. The reminiscences recently published by a fellow-prisoner of our

colleague, the reminiscences of M. Beugnot, will enable us to penetrate into the Conciergerie, on the morning of that inauspicious day.

Bailly had risen early, after having slept as usual, the sleep of the just. He took some chocolate, and conversed a long time with his nephew. The young man was a prey to despair, but the illustrious prisoner preserved all his serenity. The previous evening in returning from the Tribunal, he remarked, with admirable coolness, though springing from a certain disquietude, "that the spectators of his trial had been strongly excited against him. I fear," he added, "that the mere execution of the sentence will no longer satisfy them, which might be dangerous in its consequences. Perhaps the police will provide against it." These reflections having recurred to Bailly's mind on the 12th, he asked for, and drank hastily, two cups of coffee without milk. These precautions were a sinister omen. To his friends who surrounded him at this awful moment, and were sobbing aloud, he said, "Be calm; I have rather a difficult journey to perform, and I distrust my constitution. Coffee excites and reanimates; I hope, however, to reach the end properly."

Noon had just struck. Bailly addressed a last and tender adieu to his companions in captivity, wished them a better fate, followed the executioner without weakness as well as without bravado, mounted the fatal cart, his hands tied behind his back. Our colleague was accustomed to say: "We must entertain a bad opinion of those who, in their dying moments, have not a look to cast behind them." Bailly's last look was towards his wife. A gendarme of the escort feelingly listened to his last words, and faithfully repeated them to his widow. The procession reached the entrance to the Champ de Mars, on the side towards the river, at a quarter past one o'clock. This was the place where, according to the words of the sentence, the scaffold had been raised. The blinded crowd collected there, furiously exclaimed that the sacred ground of the Champ de la Fédération should not be soiled by the presence and by the blood of him whom they called a great criminal. Upon their demand (I had almost said their orders), the scaffold was taken down again, and carried piecemeal into one of the fosses, where it was put up a-fresh. Bailly remained the stern witness of these frightful preparations, and of these infernal clamours. Not one complaint escaped from his lips. Rain had been falling all the morning; it was cold; it drenched the body, and especially the bare head, of the venerable man. A wretch saw that he was shivering, and cried out to him, "*Thou tremblest, Bailly.*"—"*I am cold, my friend,*" mildly answered the victim. These were his last words.

Bailly descended into the moat, where the executioner burnt before him the red flag of the 17th July; he then with a firm step mounted the scaffold. Let us have the courage to say it, when the head of our venerable colleague fell, the paid witnesses whom this horrid execution had assembled on the Champ de Mars burst into infamous acclamations.

I had announced a faithful recital of the martyrdom of Bailly; I have kept my word. I said that I should banish many circumstances without reality, and that the drama would thus become less atrocious. If I am to trust your aspect, I have not accomplished the second part of my promise. The imagination perhaps cannot reach beyond the cruel facts on which I have been obliged to dilate. You ask what I can have retrenched from former relations, whilst what remains is so deplorable.

The order for execution addressed by Fouquier Tinville to the executioner has been seen by several persons now living. They all declare that if it differs from the numerous orders of a similar nature that the wretch sent off daily, it was only by the substitution of the following words: "Esplanade du Champ de Mars," for the usual designation of "Place de la Revolution." Now, the Revolutionary Tribunal has deserved many anathemas, but I never remarked its being reproached with not having known how to enforce obedience.

I felt myself relieved from an immense weight, Gentlemen, when I could dispel from my thoughts the image of a melancholy march on foot of two hours, because with it there disappeared two hours of corporeal ill-usage, which, according to those same accounts, our virtuous colleague must have endured from the Conciergerie to the Champ de Mars.

An illustrious writer asserts that they conducted Bailly to the Place de la Revolution, that the scaffold there was taken to pieces on the multitude demanding it, and that the victim was then led to the Champ de Mars. This relation is not correct. The sentence expressed in positive terms, that as an exception, the Square of the Revolution was not to be the scene of Bailly's execution. The procession went direct to the place designated.

The historian already quoted affirms that the scaffold on being put up again on the bank of the Seine was erected on a heap of rubbish; that this operation lasted some hours, and that Bailly meanwhile was drawn round the Champ de Mars several times.

These promenades are imaginary. Those men who on the arrival of the lugubrious procession vociferated that the presence of the old Mayor of Paris would soil the Champ de la Fédération, could not the next minute force him to make the circuit of it. In fact, the illustrious victim remained in the road. The cruel

dea, so knowingly attributed to the actors of those hideous cenes, to raise the fatal instrument on a heap of rubbish on the iver bank, so that Bailly might in his last moments see the house it Chaillot where he had composed his works, was so far from occurring to the mind of the multitude, that the sentence was executed in the moat between two walls.

I have not thought it my duty, Gentlemen, to represent the condemned man forced to carry some parts of the scaffold himself, because he had his hands tied behind his back. In my recital nobody waves the burning red flag over Bailly's head, because this barbarity is not mentioned in the narratives, otherwise so shocking, drawn up by some friends of our colleague shortly after the event; nor have I consented, with the author of *The History of the French Revolution,* to represent one of the soldiers forming the escort asking the question that led the victim to make, we must say so, the theatrical answer: "Yes, I tremble, but it is with cold;" but the more touching answer, so characteristic of Bailly; "Yes, my friend, I am cold."

Far be it from me, Gentlemen, to suppose that no soldier in the world would be capable of a despicable and culpable act. I do not ask, assuredly, the suppression of all courts martial; but to be induced to attribute to a man dressed in a military uniform, a personal part in this frightful drama, proofs or contemporary testimonies would be required, of which I have found no trace.

If the fact had occurred, its results would certainly have become known to the public. I take to witness an event which is found related in Bailly's Memoirs.

On the 22nd of July 1789, on the square of the Hôtel-de-Ville, a dragoon with his sabre mutilated the corpse of Berthier. His comrades, feeling outraged by this barbarity, all showed themselves instantly resolved to fight him in succession, and so wash out in his blood the disgrace he had thrown on the whole corps. The dragoon fought that same evening and was killed.

In his *History of Prisons,* Riouffe says that "Bailly exhausted the ferocity of the populace, of whom he had been the idol, and was basely abandoned by the people, though they had never ceased to esteem him."

Nearly the same idea is found expressed in *The History of the Revolution* and in several other works.

What is called the populace rarely read and did not write. To attack it and calumniate it therefore was a convenient thing, since no refutation need to be feared. I am far from supposing that the historians whose works I have quoted, ever gave way to such considerations; but I affirm, with entire certainty, that they have deceived themselves. In the sanguinary drama that has

been unrolled before your eyes, the atrocities had a quite different source from the sentiments common to the barbarians that were swarming in the dregs of society and always ready to soil it with every crime; in plainer words, it is not to the unfortunate people who have neither property, nor capital, living by the work of their hands, to the *prolétaires*, that we are to impute the deplorable incidents which marked Bailly's last moments. To put forward an opinion so remote from received opinions, is imposing on oneself the duty of proving its truth.

After his condemnation, our colleague exclaimed, says La Fayette: "I die for the sitting of the Jeu de Paume, and not for the fatal day at the Champ de Mars." I do not here intend to expound these mysterious words in the glimpses they give us by a half-light; but, whatever meaning we may attribute to them, it is evident that the sentiments and passions of the lower class have no share in them; it is a point beyond discussion.

On re-entering the Conciergerie, the evening before his death, Bailly spoke of the efforts that must have been made to excite the passions of the auditors, who followed the various phases of his trial. Factitious excitement is always the produce of corruption. The working classes are without money; they then cannot have been the corruptors or direct promoters of the distressing scenes of which Bailly complained.

The implacable enemies of the former President of the National Assembly had procured for pay some auxiliaries among the turnkeys of the Conciergerie. M. Beugnot informs us that when the venerable magistrate was consigned to the gendarmes who were to conduct him to the Tribunal, "these wretches pushed him violently, sending him from one to the other like a drunken man, calling out: *Hold there, Bailly! Catch, Bailly, there!* and that they laughed and shouted at the grave demeanour the philosopher maintained amidst the insults of those cannibals."

To confirm my statement that these violences (in comparison with which, in truth, those of the Champ de Mars lose their virulence,) were fomented by pay, I have more than the formal declaration of our colleague's fellow prisoner. For in fact I find that no other prisoner or convict underwent such treatment; not even the man called the Admiral, when he was taken to the Conciergerie for having attempted to assassinate Collot-d'Herbois.

Besides, it is not only on indirect considerations that my decided opinion is founded relative to the intervention of rich and influential people in those scenes of indescribable barbarity on the Champ de Mars. Mérard St. Just, the intimate friend of Bailly, has alluded by his initials to a wretch who the very day of our colleague's death publicly boasted of having electrified the few acolytes who,

together with him, insisted on the removal of the scaffold; the day after the execution, the meeting of the Jacobins re-echoed with the name of another individual of the Gros Caillou, who also claimed his share of influence in the crime.

I have progressively unrolled before you the series of events in our revolution, in which Bailly took an active part; I have scrupulously searched out the smallest circumstances of the deplorable affair on the Champ de Mars; I have followed our colleague in his proscription to the Revolutionary Tribunal, and to the foot of the scaffold. We had seen him before, surrounded by esteem, by respect, and by glory, in the bosom of our principal academies. Yet the work is not complete; several essential traits are still wanting.

I will therefore claim a few more minutes of your kind attention. The moral life of Bailly is like those masterpieces of ancient sculpture, that deserve to be studied in every point of view, and in which new beauties are continually discovered, in proportion as the contemplation is prolonged.

PORTRAIT OF BAILLY. — HIS WIFE.

Nature did not endow Bailly generously with those exterior advantages that please us at first sight. He was tall and thin. His visage compressed, his eyes small and sunk, his nose regular but of unusual length, and a very brown complexion, constituted an imposing whole, severe and almost glacial. Fortunately, it was easy to perceive through this rough bark, the inexhaustible benevolence of the good man; the kindness that always accompanies a serene mind, and even some rudiments of gaiety.

Bailly early endeavoured to model his conduct on that of the Abbé de Lacaille, who directed his first steps in the career of astronomy. And therefore it will be found that in transcribing five or six lines of the very feeling eulogy that the pupil dedicated to the memory of his revered master, I shall have made known at the same time many of the characteristic traits of the panegyrist:

"He was cold and reserved towards those of whom he knew little; but gentle, simple, equable, and familiar in the intercourse of friendship. It is there that, throwing off the grave exterior which he wore in public, he gave himself up to a peaceful and amiable gaiety."

The resemblance between Bailly and Lacaille goes no farther. Bailly informs us that the great astronomer proclaimed truth on all occasions, without disquieting himself as to whom it might wound. He would not consent to put vice at its ease, saying:

"If good men thus showed their indignation, bad men being known, and vice unmasked, could no longer do harm, and virtue would be more respected." This Spartan morality could not accord with Bailly's character; he admired but did not adopt it.

Tacitus took as a motto: "To say nothing false, to omit nothing true." Our colleague contented himself in society with the first half of the precept. Never did mockery, bitterness, or severity issue from his lips. His manners were a medium between those of Lacaille and the manners of another academician who had succeeded in not making a single enemy, by adopting the two axioms: "Everything is possible, and everybody is in the right."

Crébillon obtained permission from the French Academy to make his reception discourse in verse. At the moment when that poet, then almost sixty years of age, said, speaking of himself,

"No gall has ever poisoned my pen,"

the hall re-echoed with approbation.

I was going to apply this line by the author of *Rhadamistus* to our colleague, when accident offered to my sight a passage in which Lalande reproaches Bailly for having swerved from his usual character, in 1773, in a discussion that they had together on a point in the theory of Jupiter's Satellites. I set about the search for this discussion; I found the article by Bailly in a journal of that epoch, and I affirm that this dispute does not contain a word but what is in harmony with all our colleague's published writings. I return therefore to my former idea, and say of Bailly, with perfect confidence,

"No gall had ever poisoned his pen."

Diffidence is usually the trait that the biographers of studious men endeavour most to put in high relief. I dare assert, that in the common acceptation, this is pure flattery. To merit the epithet of diffident, must we think ourselves beneath the competitors of whom we are at least the equals? Must we, in examining ourselves, fail in the tact, in the intelligence, in the judgment, that nature has awarded us, and of which we make so good a use in appreciating the works of others? Oh! then, few learned men can be said to be diffident. Look at Newton: his diffidence is almost as celebrated as his genius. Well, I will extract from two of his letters, scarcely known, two paragraphs which, put side by side, will excite some surprise; the first confirms the general opinion; the second seems with equal force to contradict it. Here are the two passages:

"We are diffident in the presence of Nature."

"We may nobly feel our own strength in the face of man's vorks."

In my opinion, the opposition in these two passages is only ıpparent; it will be explained by means of a distinction which I ıave already slightly indicated.

Bailly's diffidence required the same distinction. When people ›raised him to his face on the diversity of his knowledge, our ›olleague did not immediately repel the compliment; but soon .fter, he would stop his panegyrist, and whisper in his ear with ın air of mystery: "I will confide a secret to you, pray do not ake advantage of it: I am only a very little less ignorant than ınother man."

Never did a man act more in harmony with his principles. 3ailly was led to reprimand severely a man belonging to the ıumblest and poorest class of society. Anger does not make him 'orget that he speaks to a citizen, to a man. "I ask pardon," says he first magistrate of the capital, addressing himself to a rag-gatherer; "I ask your pardon, if I am angry; but your conduct s so reprehensible, that I cannot speak to you otherwise."

Bailly's friends were wont to say that he devoted too much of ıis patrimony to pleasure. This word was calumniously inter-›reted. M. Mérard Saint Just has given the true sense of it: ' Bailly's pleasure was beneficence."

So eminent a mind could not fail to be tolerant. Such in fact 3ailly constantly showed himself in politics, and what is almost :qually rare, in regard to religion. In the month of June 1791, ıe checked in severe terms the fury with which the multitude .ppeared to be excited, at the report that at the Théatines some ›ersons had taken the Communion two or three times in one day. ' The accusation is undoubtedly false," said the Mayor of Paris; ' but if it were true, the public would not have a right to inquire nto it. Every one should have the free choice of his religion .nd his creed." Nothing would have been wanting in the pic-ure, if Bailly had taken the trouble to remark how strange it vas, that these violent scruples against repeated Communions ema-ıated from persons who probably never took the Sacrament at all.

The reports on animal magnetism, on the hospitals, on the laughterhouses, had carried Bailly's name into regions, whence he courtiers knew very cleverly how to discard true merit. *Madame* then wished to attach the illustrious academician to her ›erson as a cabinet secretary. Bailly accepted. It was an ntirely honorary title. The secretary saw the princess only once, hat was on the day of his presentation.

Were more important functions reserved for him? We must

suppose so; for some influential persons offered to procure Bailly a title of nobility and a decoration. This time the philosopher flatly refused, saying, in answer to the earnest negotiators: "I thank you, but he who has the honour of belonging to the three principal academies of France is sufficiently decorated, sufficiently noble in the eyes of rational men; a cordon, or a title, could add nothing to him."

The first secretary of the Academy of Sciences had, some years before, acted as Bailly did. Only he gave his refusal in such strong terms, that I could not easily believe them to have been written by the timid pen of Fontenelle, if I did not find them in a perfectly authentic document, in which he says: "Of all the titles in this world, I have never had any but of one sort, the titles of Academician, and they have not been profaned by an admixture of any others, more worldly and more ostentatious."

Bailly married, in November 1787, an intimate friend of his mother's, already a widow, only two years younger than himself. Madame Bailly, a distant relation of the author of the *Marseillaise*, had an attachment for her husband that bordered on adoration. She lavished on him the most tender and affectionate attention. The success that Madame Bailly might have had in the fashionable world by her beauty, her grace, by her ineffable goodness, did not tempt her. She lived in almost absolute retirement, even when the learned academician was most in society. The Mayor's wife appeared only at one public ceremony: the day of the benediction of the colours of the sixty battalions of the National Guard by the Archbishop of Paris, she accompanied Madame de Lafayette to the Cathedral. She said: "My husband's duty is to show himself in public wherever there is any good to be done, or sound advice to be given; mine is to remain at home." This rare retiring and respectable conduct did not disarm some hideous pamphleteers. Their impudent sarcasms were continually attacking the modest wife on her domestic hearth, and troubling her peace of mind. In their logic of the tavern they fancied that an elegant and handsome woman, who avoided society, could not fail to be ignorant and stupid. Thence arose a thousand imaginary stories, ridiculous both as to their matter and form, thrown out daily to the public, more, indeed, to offend and disgust the upright magistrate than to humble his companion.

The axe that ended our colleague's life, with the same stroke, and almost as completely, crushed in Madame Bailly, after so many poignant agitations and unexampled misfortunes, all that was left of strength of mind and power of intellect. A strange incident also aggravated the sadness of Madame Bailly's situation. On a

day of trouble, during her husband's lifetime, she had placed the assignats resulting from the sale of their house at Chaillot, amounting to about thirty thousand francs, in the wadding of a dress. The enfeebled memory of the unfortunate widow did not recall to her the existence of this treasure, even in the time of her greatest distress. When the age of the material which had secreted them began to reveal them to daylight, they were no longer of any value.

The widow of the author of one of the best works of the age, of the learned member of our three great academies, of the first President of the National Assembly, of the first Mayor of Paris, found herself thus reduced, by an unheard-of turn of fortune, to implore help from public pity. It was the geometer Cousin, member of this academy, who by his incessant solicitations got Madame Bailly's name inserted at the Board of Charity in his arrondissement. The support was distributed in kind. Cousin used to receive the articles at the Hôtel-de-Ville, where he was a Municipal Councillor, and carried them himself to the street de la Sourdière. It was, in short, in the street de la Sourdière that Madame Bailly had obtained two rooms gratis, in the house of a compassionate person, whose name I very much regret not having learnt. Does it not appear to you, Gentlemen, that the academician Cousin, who crossed the whole of Paris, with the bread under his arm and the meat and the candle, intended for the unfortunate widow of an illustrious colleague, did himself more honour than if he had come to one of the sittings bringing in his portfolio the results of some fine scientific research? Such noble actions are certainly worth good "Papers."

Affairs proceeded thus up to the revolution of the 18th Brumaire. On the 21st, the public criers were announcing everywhere, even in the street de la Sourdière, that General Bonaparte was Consul, and M. de Laplace Minister of the Interior. This name, so well known by the respectable widow, reached even the room that she inhabited, and caused her some emotion. That same evening, the new minister (this was a noble beginning, Gentlemen), asked for a pension of 2000 francs for Madame Bailly. The Consul granted the demand, adding to it this express condition, that the first half year should be paid in advance, and immediately. Early on the 22nd, a carriage stopped in the street de la Sourdière; Madame de Laplace descends from it, carrying in her hand a purse filled with gold. She rushed to the staircase, runs to the humble abode, that had now for several years witnessed irremediable sorrow and severe misery; Madame Bailly was at the window: "My dear friend, what are you doing

there so early?" exclaimed the wife of the minister. "Madam," replied the widow, "I heard the public crier yesterday, and I was expecting you!"

If after having, from a sense of duty, expatiated upon anarchical, odious, and sanguinary scenes, the historian of our civil discords has the good fortune to meet on his progress with an incident that gratifies the mind, raises the soul, and fills the heart with pleasing emotions, he stops there, Gentlemen, as the African traveller halts in an oasis!

HERSCHEL.

William Herschel, one of the greatest astronomers that ever lived in any age or country, was born at Hanover, on the 15th of November, 1738. The name of Herschel has become too illustrious for people to neglect searching back, up the stream of time, to learn the social position of the families that have borne it. Yet the just curiosity of the learned world on this subject has not been entirely satisfied. We only know that Abraham Herschel, great-grandfather of the astronomer, resided at Mähren, whence he was expelled on account of his strong attachment to the Protestant faith; that Abraham's son Isaac was a farmer in the vicinity of Leipzig; that Isaac's eldest son, Jacob Herschel, resisted his father's earnest desire to see him devote himself to agriculture, that he determined on being a musician, and settled at Hanover.

Jacob Herschel, father of William, the astronomer, was an eminent musician; nor was he less remarkable for the good qualities of his heart and of his mind. His very limited means did not enable him to bestow a complete education on his family, consisting of six boys and four girls. But at least, by his care, his ten children all became excellent musicians. The eldest, Jacob, even acquired a rare degree of ability, which procured for him the appointment of Master of the Band in a Hanoverian regiment, which he accompanied to England. The third son, William, remained under his father's roof. Without neglecting the fine arts, he took lessons in the French language, and devoted himself to the study of metaphysics, for which he retained a taste to his latest day.

In 1759, William Herschel, then about twenty-one years old, went over to England, not with his father, as has been erroneously published, but with his brother Jacob, whose connections in that country seemed likely to favour the young man's opening prospects in life. Still, neither London nor the country towns af-

forded him any resource in the beginning, and the first two or three years after his expatriation were marked by some cruel privations, which, however, were nobly endured. A fortunate chance finally raised the poor Hanoverian to a better position; Lord Durham engaged him as Master of the Band in an English regiment which was quartered on the borders of Scotland. From this moment the musician Herschel acquired a reputation that spread gradually, and in the year 1765 he was appointed organist at Halifax (Yorkshire). The emoluments of this situation, together with giving private lessons both in the town and the country around, procured a degree of comfort for the young William. He availed himself of it to remedy, or rather to complete, his early education. It was then that he learnt Latin and Italian, though without any other help than a grammar and a dictionary. It was then also that he taught himself something of Greek. So great was the desire for knowledge with which he was inspired while residing at Halifax, that Herschel found means to continue his hard philological exercises, and at the same time to study deeply the learned but very obscure mathematical work on the theory of music by R. Smith. This treatise, either explicitly or implicitly, supposed the reader to possess some knowledge of algebra and of geometry, which Herschel did not possess, but of which he made himself master in a very short time.

In 1766, Herschel obtained the appointment of organist to the Octagon Chapel at Bath. This was a more lucrative post than that of Halifax, but new obligations also devolved on the able pianist. He had to play incessantly either at the Oratorios, or in the rooms at the baths, at the theatre, and in the public concerts. Then, being immersed in the most fashionable circle in England, Herschel could no longer refuse the numerous pupils who wished to be instructed in his school. It is difficult to imagine how, among so many duties, so many distractions of various kinds, Herschel could continue so many studies, which already at Halifax had required in him so much resolution, so much perseverance, and a very uncommon degree of talent. We have already seen that it was by music that Herschel was led to mathematics; mathematics in their turn led him to optics, the principal and fertile source of his illustrious career. The hour finally struck, when his theoretic knowledge was to guide the young musician into a laborious application of principles quite foreign to his habits; and the brilliant success of which, as well as their excessive hardihood, will excite reasonable astonishment.

A telescope, a simple telescope, only two English feet in length, falls into the hands of Herschel during his residence at Bath. This instrument, however imperfect, shows him a multitude of

stars in the sky that the naked eye cannot discern; shows him also some of the known objects, but now under their true dimensions; reveals forms to him that the richest imaginations of antiquity had never suspected. Herschel is transported with enthusiasm. He will, without delay, have a similar instrument but of larger dimensions. The answer from London is delayed for some days: these few days appear as many centuries to him. When the answer arrives, the price that the optician demands proves to be much beyond the pecuniary resources of a mere organist. To any other man this would have been a clap of thunder. This unexpected difficulty on the contrary, inspired Herschel with fresh energy: he cannot buy a telescope, then he will construct one with his own hands. The musician of the Octagon Chapel rushes immediately into a multitude of experiments, on metallic alloys that reflect light with the greatest intensity, on the means of giving the parabolic figure to the mirrors, on the causes that in the operation of polishing affect the regularity of the figure, &c. So rare a degree of perseverance at last receives its reward. In 1774 Herschel has the happiness of being able to examine the heavens with a Newtonian telescope of five English feet focus, entirely made by himself. This success tempts him to undertake still more difficult enterprises. Other telescopes of seven, of eight, of ten, and even of twenty feet focal distance, crown his efforts. As if to answer in advance those critics who would have accused him of a superfluity of apparatus, of unnecessary luxury, in the large size of the new instruments, and his extreme minutiæ in their execution, Nature granted to the astronomical musician, on the 13th of March 1781, the unheard-of honour of commencing his career of observation with the discovery of a new planet, situated on the confines of our solar system. Dating from that moment, Herschel's reputation, no longer in his character of musician, but as a constructor of telescopes and as an astronomer, spread throughout the world. The King, George III., a great lover of science, and much inclined besides to protect and patronise both men and things of Hanoverian origin, had Herschel presented to him; he was charmed with the simple yet lucid and modest account that he gave of his repeated endeavours; he caught a glimpse of the glory that so penetrating an observer might reflect on his reign, ensured to him a pension of 300 guineas a year, and moreover a residence near Windsor Castle, first at Clay Hall and then at Slough. The visions of George III. were completely realised. We may confidently assert, relative to the little house and garden of Slough, that it is the spot of all the world where the greatest number of discoveries have been made. The name of that village will never perish: science will transmit it religiously to our latest posterity.

I will avail myself of this opportunity to rectify a mistake, of which ignorance and idleness wish to make a triumphant handle, or, at all events, to wield in their cause as an irresistible justification. It has been repeated to satiety, that at the time when Herschel entered on his astronomical career he knew nothing of mathematics. But I have already said, that during his residence at Bath, the organist of the Octagon Chapel had familiarised himself with the principles of geometry and algebra; and a still more positive proof of this is, that a difficult question on the vibration of strings loaded with small weights had been proposed for discussion in 1779: Herschel undertook to solve it, and his dissertation was inserted in several scientific collections of the year 1780.

The anecdotic life of Herschel, however, is now closed. The great astronomer will not quit his observatory any more, except to go and submit the sublime results of his laborious vigils to the Royal Society of London. These results are contained in his memoirs; they constitute one of the principal riches of the celebrated collection known under the title of *Philosophical Transactions.*

Herschel belonged to the principal Academies of Europe, and about 1816 he was named Knight of the Guelphic order of Hanover. According to the English habit, from the time of that nomination the title of Sir William took the place, in all this illustrious astronomer's memoirs, already honoured with so much celebrity, of the former appellation of Doctor William. Herschel had been named a Doctor (of laws) in the University of Oxford in 1786. This dignity, by special favour, was conferred on him without any of the obligatory formalities of examination, disputation, or pecuniary contribution, usual in that learned corporation.

I should wound the elevated sentiments that Herschel professed all his life, if I were not here to mention two indefatigable assistants that this fortunate astronomer found in his own family. The one was Alexander Herschel, endowed with a remarkable talent for mechanism, always at his brother's orders, and who enabled him to realise without delay any ideas that he had conceived*; the other was Miss Caroline Herschel, who deserves a still more particular and detailed mention.

Miss Caroline Lucretia Herschel went to England as soon as her brother became special astronomer to the king. She received the appellation there of Assistant Astronomer, with a moderate salary. From that moment she unreservedly devoted herself to

* When age and infirmities obliged Alexander Herschel to give up his profession as a musician, he quitted Bath, and returned to Hanover, very generously provided by Sir William with a comfortable independence for life.

he service of her brother, happy in contributing night and day to .is rapidly increasing scientific reputation. Miss Caroline shared n all the night-watches of her brother, with her eye constantly on he clock, and the pencil in her hand; she made all the calcula-ions without exception; she made three or four copies of all the bservations in separate registers; co-ordinated, classed and nalyzed them. If the scientific world saw with astonishment iow Herschel's works succeeded each other with unexampled 'apidity during so many years, they were specially indebted for it o the ardour of Miss Caroline. Astronomy, moreover, has been lirectly enriched by several comets through this excellent and ·espectable lady. After the death of her illustrious brother, Miss Caroline retired to Hanover, to the house of Jahn Dietrich Herschel, a musician of high reputation, and the only surviving orother of the astronomer.

William Herschel died without pain on the 23rd of August 1822, iged eighty-three. Good fortune and glory never altered in him the fund of infantine candour, inexhaustible benevolence, and sweetness of character, with which nature had endowed him. He preserved to the last both his brightness of mind and vigour of intellect. For some years Herschel enjoyed with delight the distinguished success of his only son *, Sir John Herschel. At his last hour he sunk to rest with the pleasing conviction that his beloved son, heir of a great name, would not allow it to fall into oblivion, but adorn it with fresh lustre, and that great discoveries would honour his career also. No prediction of the illustrious astronomer has been more completely verified.

The English journals gave an account of the means adopted by the family of William Herschel, for preserving the remains of the great telescope of thirty-nine English feet (twelve metres) constructed by that celebrated astronomer.

The metal tube of the instrument carrying at one end the recently cleaned mirror of four feet ten inches in diameter, has been placed horizontally in the meridian line, on solid piers of masonry, in the midst of the circle, where formerly stood the mechanism requisite for manœuvring the telescope. The first of January 1840, Sir John Herschel, his wife, their children, seven in number, and some old family servants, assembled at Slough. Exactly at noon, the party walked several times in procession round the instrument; they then entered the tube of the telescope, seated themselves on benches that had been prepared for the

* Sir W. Herschel had married Mary, the widow of John Pitt, Esq., possessed of a considerable jointure, and the union proved a remarkable accession of domestic happiness. This lady survived Sir William by several years. They had but this son.—*Translator's Note.*

purpose, and sung a requiem, with English words composed by Sir John Herschel himself. After their exit, the illustrious family ranged themselves around the great tube, the opening of which was then hermetically sealed. The day concluded with a party of intimate friends.

I know not whether those persons who will only appreciate things from the peculiar point of view from which they have been accustomed to look, may think there was something strange in several of the details of the ceremony that I have just described. I affirm at least that the whole world will applaud the pious feeling which actuated Sir John Herschel; and that all the friends of science will thank him for having consecrated the humble garden where his father achieved such immortal labours, by a monument more expressive in its simplicity than pyramids or statues.

CHRONOLOGICAL TABLE

OF THE MEMOIRS OF WILLIAM HERSCHEL.*

1780. *Philosophical Transactions*, vol. lxx.—Astronomical Observations on the Periodical Star in the Neck of the Whale.—Astronomical Observations relative to the Lunar Mountains.

1781. *Phil. Trans.*, vol. lxxi.—Astronomical Observations on the Rotation of the Planets on their Axes, made with a View to decide whether the Daily Rotation of the Earth be always the same.—On the Comet of 1781, afterwards called the *Georgium Sidus*.

1782. *Phil. Trans.*, vol. lxxii.—On the Parallax of the Fixed Stars.—Catalogue of Double Stars.—Description of a Lamp Micrometer, and the Method of using it.—Answers to the Doubts that might be raised to the high magnifying Powers used by Herschel.

1783. *Phil. Trans.*, vol. lxxiii.—Letter to Sir Joseph Banks on the Name to be given to the new Planet.—On the Diameter of the Georgium Sidus, followed by the Description of a Micrometer with luminous or dark Discs.—On the proper Motion of the Solar System, and the various Changes that have occurred among the Fixed Stars since the Time of Flamsteed.

1784. *Phil. Trans.*, vol. lxxiv.—On some remarkable Appearances in the Polar Regions of Mars, the Inclination of its Axis, the Position of its Poles, and its Spheroïdal Form.—Some Details on the real Diameter of Mars, and on its Atmosphere.—Analysis of some Observations on the Constitution of the Heavens.

1785. *Phil. Trans.*, vol. lxxv.—Catalogue of Double Stars.—On the Constitution of the Heavens.

1786. *Phil. Trans.*, vol. lxxvi.—Catalogue of a Thousand Nebulæ and Clusters of Stars.—Researches on the Cause of a Defect of Definition in Vision, which has been attributed to the Smallness of the Optic Pencils.

1787. *Phil. Trans*, vol. lxxvii.—Remarks on the new Comet.—Discovery of Two Satellites revolving round George's Planet.—On Three Volcanos in the Moon.

* These titles are copied direct from the Philosophical Transactions, instead of being retranslated.—*Translator's Note.*

1788. *Phil. Trans.*, vol. lxxviii.— On George's Planet (Uranus) and its Satellites.

1789. *Phil. Trans.*, vol. lxxix.— Observations on a Comet. Catalogue of a Second Thousand new Nebulæ and Clusters of Stars.— Some Preliminary Remarks on the Constitution of the Heavens.

1790. *Phil Trans.*, vol. lxxx.— Discovery of Saturn's Sixth and Seventh Satellites; with Remarks on the Constitution of the Ring, on the Planet's Rotation round an Axis, on its Spheroïdal Form, and on its Atmosphere.— On Saturn's Satellites, and the Rotation of the Ring round an Axis.

1791. *Phil. Trans.*, vol. lxxxi.—On the Nebulous Stars and the Suitableness of this Epithet.

1792. *Phil. Trans.*, vol. lxxxii.— On Saturn's Ring, and the Rotation of the Planet's Fifth Satellite round an Axis.— Mixed Observations.

1793. *Phil. Trans*, vol. lxxxiii.— Observations on the Planet Venus.

1794. *Phil. Trans.*, vol. lxxxiv.— Observations on a Quintuple Band in Saturn.— On some Peculiarities observed during the last Solar Eclipse.— On Saturn's Rotation round an Axis.

1795. *Phil. Trans.*, vol. lxxxv.— On the Nature and Physical Constitution of the Sun and Stars.— Description of a Reflecting Telescope forty feet in length.

1796. *Phil. Trans.*, vol. lxxxvi.— Method of observing the Changes that happen to the Fixed Stars; Remarks on the Stability of our Sun's Light.— Catalogue of Comparative Brightness, to determine the Permanency of the Lustre of Stars.— On the Periodical Star α Herculis, with Remarks tending to establish the Rotatory Motion of the Stars on their Axes; to which is added a second Catalogue of the Brightness of the Stars.

1797. *Phil. Trans.*, vol. lxxxvii.— A Third Catalogue of the comparative Brightness of the Stars; with an Introductory Account of an Index to Mr Flamsteed's Observations of the Fixed Stars, contained in the Second Volume of the Historia Cœlestis, to which are added several useful Results derived from that Index.— Observations of the changeable Brightness of the Satellite of Jupiter, and of the Variation in their apparent Magnitudes; with a Determination of the Time of their rotary Motions on their Axes, to which is added a Measure of the Diameter of the Second Satellite, and an Estimate of the comparative Size of the Fourth.

1798. *Phil. Trans.*, vol. lxxxviii.— On the Discovery of Four additional Satellites of the Georgium Sidus. The retrograde Motion of its old Satellite announced; and the Cause of their Disappearance at certain Distances from the Planet explained.

1799. *Phil. Trans.*, vol. lxxxix.— A Fourth Catalogue of the comparative Brightness of the Stars.

1800. *Phil. Trans.*, vol. xc.— On the Power of penetrating into Space by Telescopes, with a comparative Determination of the Extent of that Power in Natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations.— Investigation of the Powers of the Prismatic Colours to heat and illuminate Objects; with Remarks that prove the different Refrangibility of radiant Heat; to which is added an Inquiry into the Method of viewing the Sun advantageously with Telescopes of large Apertures and high magnifying Powers.— Experiments on the Refrangibility of the Invisible Rays of the Sun.— Experiments on the Solar and on the Terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat or rather the Rays which occasion them, are subject, in order to determine whether they are the same or different.

1801. *Phil. Trans.*, vol. xci.— Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission

of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations.—Additional Observations tending to investigate the Symptoms of the variable Emission of the Light and Heat of the Sun; with Trials to set aside darkening Glasses, by transmitting the Solar Rays through Liquids, and a few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper.

1802. *Phil. Trans.*, vol. xcii.— Observations on the two lately discovered celestial Bodies (Ceres and Pallas).— Catalogue of 500 new Nebulæ and Clusters of Stars, with Remarks on the Construction of the Heavens.

1803. *Phil. Trans.*, vol. xciii.— Observations of the Transit of Mercury over the Disc of the Sun; to which is added an Investigation of the Causes which often prevent the proper Action of Mirrors.—Account of the Changes that have happened during the last Twenty-five Years in the relative Situation of Double Stars; with an Investigation of the Cause to which they are owing.

1804. *Phil. Trans.*, vol. xciv.— Continuation of an Account of the Changes that have happened in the relative Situation of Double Stars.

1805. *Phil. Trans.*, vol. xcv. — Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of Celestial and Terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr. Harding's lately discovered Star.— On the Direction and Velocity of the Motion of the Sun and Solar System.— Observation on the singular Figure of the Planet Saturn.

1806. *Phil. Trans.*, vol. xcvi.— On the Quantity and Velocity of the Solar Motion.— Observations on the Figure, the Climate, and the Atmosphere of Saturn and its Ring.

1807. *Phil. Trans.*, vol. xcvii.— Experiments for investigating the Cause of the Coloured Concentric Rings, discovered by Sir Isaac Newton between two Object-glasses laid one upon another.— Observations on the Nature of the new celestial Body discovered by Dr. Olbers, and of the Comet which was expected to appear last January in its Return from the Sun.

1808. *Phil. Trans.*, vol. xcviii.— Observations of a Comet, made with a view to investigate its Magnitude, and the Nature of its Illumination. To which is added, an Account of a new Irregularity lately perceived in the Apparent Figure of the Planet Saturn.

1809. *Phil. Trans.*, vol. xcix. — Continuation of Experiments for investigating the Cause of Coloured Concentric Rings, and other Appearances of a similar Nature.

1810. *Phil. Trans.*, vol. c.— Supplement to the First and Second Part of the Paper of Experiments for investigating the Cause of Coloured Concentric Rings between Object Glasses, and other Appearances of a similar Nature.

1811. *Phil. Trans.*, vol. ci.—Astronomical Observations relating to the Construction of the Heavens, arranged for the Purpose of a critical Examination, the Result of which appears to throw some new Light upon the Organisation of the Celestial Bodies.

1812. *Phil. Trans.*, vol. cii.— Observations of a Comet, with Remarks on the Construction of its different Parts.— Observations of a Second Comet, with Remarks on its Construction.

1814. *Phil. Trans.*, vol. civ.—Astronomical Observations relating to the Sidereal Part of the Heavens, and its Connection with the Nebulous Part; arranged for the Purpose of a critical Examination.

1815. *Phil. Trans.*, vol. cv.— A Series of Observations of the Satellites of the Georgian Planet, including a Passage through the Node of their Orbits; with an Introductory Account of the Telescopic Apparatus that has been used

on this Occasion, and a final Exposition of some calculated Particulars deduced from the Observations.

1817. *Phil. Trans.*, vol. cvii.—Astronomical Observations and Experiments tending to investigate the Local Arrangement of the Celestial Bodies in Space, and to determine the Extent and Condition of the Milky Way.

1818. *Phil. Trans.*, vol. cviii.—Astronomical Observations and Experiments selected for the Purpose of ascertaining the relative Distances of Clusters of Stars, and of investigating how far the Power of Telescopes may be expected to reach into Space, when directed to ambiguous Celestial Objects.

1822. *Memoirs of the Astronomical Society of London.*—On the Positions of 145 new Double Stars.

The chronological and detailed analysis of so many labours would throw us into numerous repetitions. A systematic order will be preferable; it will more distinctly fix the eminent place that Herschel will never cease to occupy in the small group of our contemporary men of genius, whilst his name will re-echo to the most distant posterity. The variety and splendour of Herschel's labours vie with their extent. The more we study them, the more we must admire them. It is with great men, as it is with great movements in the arts, we cannot understand them without studying them under various points of view.

Let us here again make a general reflection. The memoirs of Herschel are, for the greater part, pure and simple extracts from his inexhaustible journals of observations at Slough, accompanied by a few remarks. Such a table would not suit historical details. In these respects the author has left almost everything to his biographers to do for him. And they must impose on themselves the task of assigning to the great astronomer's predecessors the portion that legitimately belongs to them, out of the mass of discoveries, which the public (we must say) has got into an erroneous habit of referring too exclusively to Herschel.

At one time I thought of adding a note to the analysis of each of the illustrious observer's memoirs, containing a detailed indication of the improvements or corrections that the progressive march of science has brought on. But in order to avoid an exorbitant length in this biography, I have been obliged to give up my project. In general I shall content myself with pointing out what belongs to Herschel, referring to my *Treatise on Popular Astronomy* for the historical details. The life of Herschel had the rare advantage of forming an epoch in an extensive branch of astronomy; it would require us almost to write a special treatise on astronomy, to show thoroughly the importance of all the researches that are due to him.

IMPROVEMENTS IN THE MEANS OF OBSERVATION.

The improvements that Herschel made in the construction and management of telescopes have contributed so directly to the

discoveries with which that observer enriched astronomy, that we cannot hesitate to bring them forward at once.

I read the following passage in a Memoir by Lalande, printed in 1783, and forming part of the preface to vol. viii. of the *Ephemerides of the Celestial Motions.*

"Each time that Herschel undertakes to polish a mirror (of a telescope), he condemns himself to ten, or twelve, or even fourteen hours' constant work. He does not quit his workshop for a minute, not even to eat, but receives from the hands of his sister that nourishment without which one could not undergo such prolonged fatigue. Nothing in the world would induce Herschel to abandon his work; for, according to him, it would be to spoil it."

The advantages that Herschel found in 1783, 1784, and 1785 in employing telescopes of twenty feet and with large apertures, made him wish to construct much larger still. The expense would be considerable; King George III. provided for it. The work, begun about the close of 1785, was finished in August 1789. This instrument had an iron cylindrical tube, thirty-nine feet four inches English in length, and four feet ten inches in diameter. Such dimensions are enormous compared with those of telescopes made till then. They will appear but small, however, to persons who have heard the report of a pretended ball given in the Slough telescope. The propagators of this popular rumour had confounded the astronomer Herschel with the brewer Meux, and a cylinder in which a man of the smallest stature could scarcely stand upright, with certain wooden vats, as large as a house, in which beer is made and kept in London.

Herschel's telescope, forty English feet* in length, allowed of the realisation of an idea, the advantages of which would not be sufficiently appreciated if I did not here recall to mind some facts.

In any telescope, whether refracting or reflecting, there are two principal parts: the part that forms the aërial images of the distant objects, and the small lens by the aid of which these images are enlarged just as if they consisted of radiating matter. When the image is produced by means of a lenticular glass, the place it occupies will be found in the prolongation of the line that extends from the object to the centre of the lens. The astronomer, furnished with an eye-piece and wishing to examine that image, must necessarily place himself *beyond* the point where the rays that form it have crossed each other: *beyond,* let us carefully remark, means *farther off* from the object-glass. The observer's head, his

* Conforming to general usage, and to Sir W. Herschel himself, we shall allude to this instrument as the *forty-foot* telescope, though M. Arago adheres to thirty-nine feet and drops the inches, probably because the Parisian foot is rather longer than the English.—*Translator's Note.*

ody, cannot then injure the formation or the brightness of the mage, however small may be the distance from which we have to tudy it. But it is no longer thus with the image formed by neans of reflection. For the image is now placed between the bject and the reflecting mirror: and when the astronomer ap-roaches in order to examine it, he inevitably intercepts, if not he totality, at least a very considerable portion, of the luminous ays, which would otherwise have contributed to give it great plendour. It will now be understood, why in optical instruments vhere the images of distant objects are formed by the reflection f light, it has been necessary to carry the images, by the aid f a second reflection, out of the tube that contains and sustains he principal mirror. When the small mirror on the surface of vhich the second reflection is effected, is plane and inclined at an ngle of 45° to the axis of the telescope; when the image is eflected laterally through an opening made near the edge of the ube and furnished with an eye-piece; when, in a word, the astro-omer looks definitively in a direction perpendicular to the line escribed by the luminous rays coming from the object and falling n the centre of the great mirror, then the telescope is called *Vewtonian*. But in the *Gregorian* telescope, the image formed y the principal mirror falls on a second mirror, which is very mall, slightly curved, and parallel to the first. The small mirror eflects the first image and throws it beyond the large mirror, rough an opening made in the middle of that principal mirror.

Both in the one and in the other of these two telescopes, the mall mirror interposed between the object and the great mirror orms relative to the latter a sort of screen which prevents its ntire surface from contributing towards forming the image. The mall mirror also, in regard to intensity, gives some trouble.

Let us suppose, in order to clear up our ideas, that the material f which the two mirrors are made, reflects only half of the ncident light. In the course of the first reflection, the immense uantity of rays that the aperture of the telescope had received, nay be considered as reduced to half. Nor is the diminution less n the small mirror. Now, half of half is a quarter. Therefore he instrument will send to the eye of the observer only a quarter f the incident light that its aperture had received. These two auses of diminished light not existing in a refracting telescope, would give, under parity of dimensions, four times more* light han a Newtonian or Gregorian telescope gives.

Herschel did away with the small mirror in his large telescope. he large mirror is not mathematically centred in the large tube

* It would be more correct to say four times *as much* light.—*Translator.*

that contains it, but is placed rather obliquely in it. This slight obliquity causes the images to be formed not in the axis of the tube, but very near its circumference, or outer mouth, we may call it. The observer may therefore look at them there direct, merely by means of an eye-piece. A small portion of the astronomer's head, it is true, then encroaches on the tube; it forms a screen, and interrupts some incident rays. Still in a large telescope, the loss does not amount to half by a great deal; which it would inevitably do if the small mirror were there.

Those telescopes, in which the observer, placed at the anterior extremity of the tube, looks direct into the tube and turns his back to the objects, were called by Herschel *front view telescopes.* In the vol. lxxvi. of the *Philosophical Transactions* he says, that the idea of this construction occurred to him in 1776, and that he then applied it unsuccessfully to a ten-foot telescope; that during the year 1784, he again made a fruitless trial of it in a twenty-foot telescope. Yet I find that on the 7th of September 1784, he recurred to *a front view* in observing some nebulæ and groups of stars. However discordant these dates may be, we cannot without injustice neglect to remark, that a front view telescope was already described in 1732, in volume vi. of the collection entitled *Machines and Inventions approved by the Academy of Sciences.* The author of this innovation is Jaques Lemaire, who has been unduly confounded with the English Jesuit, Christopher Maire, assistant to Boscovitch, in measuring the meridian comprised between Rome and Rimini. Jaques Lemaire having only telescopes of moderate dimensions in view, was obliged, in order not to sacrifice any of the light, to place the great mirror so obliquely, that the image formed by its surface should fall entirely outside the tube of the instrument. So great a degree of inclination would certainly deform the objects. The *front view* construction is admissible only in very large telescopes.

I find in the *Transactions* for 1803, that in solar observations, Herschel sometimes employed telescopes, the great mirror of which was made of glass. It was a telescope of this sort that he used for observing the transit of Mercury on the 9th of November, 1802. It was seven English feet long, and six inches and three tenths in diameter.

Practical astronomers know how much the mounting of a telescope contributes to produce correct observations. The difficulty of a solid yet very moveable mounting, increases rapidly with the dimensions and weight of an instrument. We may then conceive that Herschel had to surmount many obstacles, to mount a telescope suitably, of which the mirror alone weighed upwards of 1000 kilogrammes (*a ton*). But he solved this problem to his

entire satisfaction by the aid of a combination of spars, of pulleys and of ropes, of all which a correct idea may be formed by referring to the woodcut we have given in our *Treatise on Popular Astronomy* (vol. i.). This great apparatus, and the entirely different stands that Herschel imagined for telescopes of smaller dimensions assign to that illustrious observer, a distinguished place amongst the most ingenious mechanics of our age.

Persons in general, I may even say the greater part of astronomers, know not what was the effect that the great forty-foot telescope had in the labours and discoveries of Herschel. Still we are not less mistaken when we fancy that the observer of Slough always used this telescope, than in maintaining with Baron von Zach (see *Monatliche Correspondenz,* January 1802), that the colossal instrument was of no use at all, that it did not contribute to any one discovery, that it must be considered as a mere object of curiosity. These assertions are distinctly contradicted by Herschel's own words. In the volume of *Philosophical Transactions* for the year 1795 (p. 350.) I read for example: "On the 28th of August 1789, having directed my telescope (of forty feet) to the heavens, I discovered the sixth satellite of Saturn, and I perceived the spots on that planet, better than I had been able to do before." (See also relative to this sixth satellite the *Philosophical Transactions* for 1790, p. 10.) In that same volume of 1790, p. 11., I find: "The great light of my forty-foot telescope was then so useful, that on the 17th of September 1789, I remarked the seventh satellite, then situated at its greatest western elongation."

The 10th of October, 1791, Herschel saw the ring of Saturn and the fourth satellite, looking in at the mirror of his forty-foot telescope, with his naked eye, without any sort of eye-piece.

Let us acknowledge the true motives that prevented Herschel from oftener using his telescope of forty feet. Notwithstanding the excellence of the mechanism, the manœuvring of that instrument required the constant aid of two labourers, and that of another person charged with noting the time at the clock. During some nights when the variation of temperature was considerable, this telescope, on account of its great mass, was always behindhand with the atmosphere in thermometric changes, which was very injurious to the distinctness of the images.

Herschel found that in England, there are not above a hundred hours in a year during which the heavens can be advantageously observed with a telescope of forty feet, furnished with a magnifying power of a thousand. This remark led the celebrated astronomer to the conclusion, that to take a complete survey of the heavens with his large instrument, though each successive

field should remain only for an instant under inspection, would not require less than eight hundred years.

Herschel explains in a very natural way the rare occurrence of the circumstances in which it is possible to make good use of a telescope of forty feet, and of very large aperture.

A telescope does not magnify real objects only, but magnifies also the apparent irregularities arising from atmospheric refractions; now, all other things being equal, these irregularities of refraction must be so much the stronger, so much the more frequent, as the stratum of air is thicker through which the rays have passed to go and form the image.

Astronomers experienced extreme surprise, when in 1782, they learned that Herschel had applied linear magnifying powers of a thousand, of twelve hundred, of two thousand two hundred, of two thousand six hundred, and even of six thousand times, to a reflecting telescope of seven feet in length. The Royal Society of London experienced this surprise, and officially requested Herschel to give publicity to the means he had adopted for ascertaining such amounts of magnifying power in his telescopes. Such was the object of a memoir that he inserted in vol. lxxii. of the *Philosophical Transactions;* and it dissipated all doubts. No one will be surprised that magnifying powers, which it would seem ought to have shown the Lunar mountains, as the chain of Mont Blanc is seen from Maçon, from Lyons, and even from Geneva, were not easily believed in. They did not know that Herschel had never used magnifying powers of three thousand, and six thousand times, except in observing brilliant stars; they had not remembered that light reflected by planetary bodies, is too feeble to continue distinct under the same degree of magnifying power as the actual light of the fixed stars does.

Opticians had given up, more from theory than from careful experiments, attempting high magnifying powers, even for reflecting telescopes. They thought that the image of a small circle cannot be distinct, cannot be sharp at the edges, unless the pencil of rays coming from the object in nearly parallel lines, and which enters the eye after having passed through the eye-piece, be sufficiently broad. This being once granted, the inference followed, that an image ceases to be well defined, when it does not strike at least two of the nervous filaments of the retina with which that organ is supposed to be overspread. These gratuitous circumstances, grafted on each other, vanished in presence of Herschel's observations. After having put himself on his guard against the effects of diffraction, that is to say, against the scattering that light undergoes when it passes the terminal angles of bodies, the illustrious astronomer proved, in 1786, that objects can be seen

well defined by means of pencils of light whose diameter does not equal five tenths of a millimetre.

Herschel looked on the almost unanimous opinion of the double lens eye-piece being preferable to the single lens eye-piece, as a very injurious prejudice in science. For experience proved to him, notwithstanding all theoretic deductions, that with equal magnifying powers, in reflecting telescopes at least, (and this restriction is of some consequence), the images were brighter and better defined with single than with double eye-pieces. On one occasion, this latter eye-piece would not show him the bands of Saturn, whilst by the aid of a single lens they were perfectly visible. Herschel said: "The double eye-piece must be left to amateurs and to those who, for some particular object, require a large field of vision." (*Philosophical Transactions,* 1782, *pages* 94 *and* 95.)

It is not only relative to the comparative merit of single or double eye-pieces that Herschel differs from the general opinions of opticians; he thinks, moreover, that he has proved by decisive experiments, that concave eye-pieces (like that used by Galileo) surpass the convex eye-piece by a great deal, both as regards clearness and definition.

Herschel assigns the date of 1776 to the experiments which he made to decide this question. (*Philosophical Transactions,* year 1815, p. 297.) Plano-concave and double concave lenses produced similar effects. In what did these lenses differ from the double convex lenses? In one particular only: the latter received the rays reflected by the large mirror of the telescope, after their union at the focus, whereas the concave lenses received the same rays before that union. When the observer made use of a convex lens, the rays that went to the back of the eye to form an image on the retina, had crossed each other before in the air; but no crossing of this kind took place when the observer used a concave lens. Holding the double advantage of this latter sort of lens over the other, as quite proved, one would be inclined, like Herschel, to admit, "that a certain mechanical effect, injurious to clearness and definition, would accompany the focal crossing of the rays of light." *

This idea of the crossing of the rays suggested an experiment to the ingenious astronomer, the result of which deserves to be recorded.

A telescope of ten English feet was directed towards an adver-

* On comparing the Cassegrain telescopes with a small convex mirror, to the Gregorian telescopes with a small concave mirror, Captain Kater found that the former, in which the luminous rays do not cross each other before falling on the small mirror, possess, as to intensity, a marked advantage over the latter, in which this crossing takes place.

tisement covered with very small printing, and placed at a sufficient distance. The convex lens of the eye-piece was carried not by a tube properly so called, but by four rigid fine wires placed at right angles. This arrangement left the focus open in almost every direction. A concave mirror was then placed so that it threw a very condensed image of the sun laterally on the very spot where the image of the advertisement was formed. The solar rays, after having crossed each other, finding nothing on their route, went on and lost themselves in space. A screen, however, allowed the rays to be intercepted at will before they united.

This done, having applied the eye to the eye-piece and directed all his attention to the telescopic image of the advertisement, Herschel did not perceive that the taking away and then replacing the screen made the least change in the brightness or definition of the letters. It was therefore of no consequence, in the one instance as well as in the other, whether the immense quantity of solar rays crossed each other at the very place where, *in another direction,* the rays united that formed the image of the letters. I have marked in Italics the words that especially show in what this curious experiment differs from the previous experiments, and yet does not entirely contradict them. In this instance the rays of various origin, those coming from the advertisement and from the sun, crossed each other respectively in almost rectangular directions; during the comparative examination of the stars with convex and with concave eye-pieces, the rays that seemed to have a mutual influence, had a common origin and crossed each other at very acute angles. There seems to be nothing, then, in the difference of the results at which we need to be much surprised.

Herschel increased the catalogue, already so extensive, of the mysteries of vision, when he explained in what manner we must endeavour to distinguish separately the two members of certain double stars very close to each other. He said, if you wish to assure yourself that η Coronæ is a double star, first direct your telescope to α Geminorum, to ζ Aquarii, to μ Draconis, to ρ Herculis, to α Piscium, to ε Lyræ. Look at those stars for a long time, so as to acquire the habit of observing such objects. Then pass on to ξ Ursæ majoris, where the closeness of the two

* In the selection of *i* Bootis as a test, Arago has taken the precaution of giving its corresponding denomination in other catalogues, and Bailey appends the following note, No. 2062, to 44 Bootis. "In the British Catalogue this star is not denoted by any letter: but Bayer calls it *i*, and on referring to the earliest MS. Catalogue in MSS. vol. xxv., I find it is there so designated; I have therefore restored the letter." (See Bailey's Edition of Flamsteed's British Catalogue of Stars, 1835.) The distance between the two members of this double star is 3″·7 and position 23°·5. See "Bedford Cycle."—*Translator.*

members is still greater. In a third essay select ι Bootis (marked 44 by Flamsteed and i in Harris's maps) *, the star that precedes a Orionis, n of the same constellation, and you will then be prepared for the more difficult observation of η Coronæ. Indeed η Coronæ is a sort of miniature of i Bootis, which may itself be considered as a miniature of a Gem. (*Philosophical Transactions*, 1782, p. 100.)

As soon as Piazzi, Olbers, and Harding had discovered three of the numerous telescopic planets now known, Herschel proposed to himself to determine their real magnitudes; but telescopes not having then been applied to the measurement of excessively small angles, it became requisite, in order to avoid any illusion, to try some experiments adapted to giving a scale of the powers of those instruments. Such was the labour of that indefatigable astronomer, of which I am going to give a compressed abridgment.

The author relates first, that in 1774, he endeavoured to ascertain experimentally, with the naked eye and at the distance of distinct vision, what angle a circle must subtend to be distinguished by its form from a square of similar dimensions. The angle was never smaller than 2′ 17″; therefore at its maximum it was about one fourteenth of the angle subtended by the diameter of the moon.

Herschel did not say, either of what nature the circles and squares of paper were that he used, nor on what background they were projected. It is a lacuna to be regretted, for in those phenomena the intensity of light must be an important feature. However it may have been, the scrupulous observer not daring to extend to telescopic vision what he had discovered relative to vision with the naked eye, he undertook to do away with all doubt, by direct observations.

On examining some pins' heads placed at a distance in the open air, with a three-foot telescope, Herschel could easily discern that those bodies were round, when the subtended angles became, after their enlargement, 2′ 19″. This is almost exactly the result obtained with the naked eye.

When the globules were darker; when instead of pins' heads, small globules of sealing wax were used, their spherical form did not begin to be distinctly visible till the moment when the subtended magnified angles, that is, the moment when the natural angle multiplied by the magnifying power, amounted to five minutes.

In a subsequent series of experiments, some globules of silver placed very far from the observer, allowed their globular form to be perceived, even when the magnified angle remained below two minutes.

Under equality of subtended angle, then, the telescopic vision with strong magnifying powers showed itself superior to the naked eye vision. This result is not unimportant.

If we take notice of the magnifying powers used by Herschel in these laborious researches, powers that often exceeded five hundred times, it will appear to be established that the telescopes possessed by modern astronomers, may serve to verify the round form of distant objects, the form of celestial bodies even when the diameters of those bodies do not subtend naturally (to the naked eye), angles of above three tenths of a second: and 500 multiplied by three tenths of a second, give 2′ 30″.

Refracting telescopes were still ill understood instruments, the result of chance, devoid of certain theory, when they already served to reveal brilliant astronomical phenomena. Their theory, in as far as it depended on geometry and optics, made rapid progress. These two early phases of the problem leave but little more to be wished for; it is not so with a third phase, hitherto a good deal neglected, connected with physiology, and with the action of light on the nervous system. Therefore, we should search in vain in old treatises on optics and on astronomy, for a strict and complete discussion on the comparative effect that the size and intensity of the images, that the magnifying power and the aperture of a telescope may have, by night and by day, on the visibility of the faintest stars. This lacuna Herschel tried to fill up in 1799; such was the aim of the memoir entitled *On the space-penetrating Power of Telescopes.*

This memoir contains excellent things; still it is far from exhausting the subject. The author, for instance, entirely overlooks the observations made by day. I also find, that the hypothetical part of the discussion is not perhaps so distinctly separated from the rigorous part as it might be; that disputable numbers, though given with a degree of precision down to the smallest decimals, do not look well as terms of comparison with some results which, on the contrary, rest on observations bearing mathematical evidence.

Whatever may be thought of these remarks, the astronomer or the physicist who would like again to undertake the question of visibility with telescopes, will find some important facts in Herschel's memoir, and some ingenious observations, well adapted to serve them as guides.

LABOURS IN SIDEREAL ASTRONOMY.

The curious phenomenon of a periodical change of intensity in certain stars, very early excited a keen attention in Herschel. The first memoir by that illustrious observer presented to the Royal Society of London and inserted in the *Philosophical Trans-*

actions treats precisely of the changes of intensity of the star *o* in the neck of the Whale.

This memoir was still dated from Bath, May 1780. Eleven years after, in the month of December 1791, Herschel communicated a second time to that celebrated English Society the remarks that he had made by sometimes directing his telescopes to the mysterious star. At both those epochs the observer's attention was chiefly applied to the absolute values of the *maxima* and *minima* of intensity.

The changeable star in the Whale was not the only periodical star with which Herschel occupied himself. His observations of 1795 and of 1796 proved that *a* Herculis also belongs to the category of variable stars, and that the time requisite for the accomplishment of all the changes of intensity, and for the star's return to any given state, was sixty days and a quarter. When Herschel obtained this result, about ten changeable stars were already known; but they were all either of very long or very short periods. The illustrious astronomer considered, that by introducing between two groups that exhibited very short and very long periods, a star of somewhat intermediate conditions,—for instance, one requiring sixty days to accomplish all its variations of intensity,—he had advanced the theory of these phenomena by an essential step: the theory at least that attributes everything to a movement of rotation round their centres which the stars may undergo.

Sir William Herschel's catalogues of double stars offer a considerable number to which he ascribes a decided green or blue tint. In binary combinations, when the small star appears very blue or very green, the large one is usually yellow or red. It does not appear that the great astronomer took sufficient interest in this circumstance. I do not find, indeed, that the almost constant association of two complementary colours (of yellow and blue, or of red and green), ever led him to suspect that one of those colours might not have anything real in it, that it often might be a mere illusion, a mere result of contrast. It was only in 1825, that I showed that there are stars whose contrast really explains their apparent colour; but I have proved besides, that blue is incontestibly the colour of certain insulated stars, or stars that have only white ones, or other blue ones in their vicinity. Red is the only colour that the ancients ever distinguished from white in their catalogues.

Herschel also endeavoured to introduce numbers in the classification of stars as to magnitude; he has endeavoured, by means of numbers, to show the comparative intensity of a star of first magnitude, with one of second, or one of third magnitude, &c.

In one of the earliest of Herschel's memoirs, we find, that the apparent sidereal diameters are proved to be for the greater part factitious, even when the best made telescopes are used. Diameters estimated by seconds, that is to say, reduced according to the magnifying power, diminish as the magnifying power is increased. These results are of the greatest importance.

In the course of his investigation of sidereal parallax, though without finding it, Herschel made an important discovery: that of the proper motion of our system. To show distinctly the direction of the motion of the solar system, not only was a displacement of the sidereal perspective required, but profound mathematical knowledge, and a peculiar tact. This peculiar tact Herschel possessed in an eminent degree. Moreover, the result deduced from the very small number of proper motions known at the beginning of 1783, has been found almost to agree with that found recently by clever astronomers, by the application of subtile analytical formulæ, to a considerable number of exact observations.

The proper motions of the stars have been known and proved for more than a century, and already Fontenelle used to say in 1738, that the sun probably also moved in a similar way. The idea of partly attributing the displacement of the stars to a motion of the sun, had suggested itself to Bradley and to Mayer. And Lambert especially had been very explicit on the subject. Until then, however, there were only conjectures and mere probabilities. Herschel passed those limits. He himself proved that the sun positively moves; and that in this respect also, that immense and dazzling body must be ranged among the stars; that the apparently inextricable irregularities of numerous sidereal proper motions arise in great measure from the displacement of the solar system; that, in short, the point of space towards which we are annually advancing, is situated in the constellation of Hercules.

These are magnificent results. The discovery of the proper motion of our system will always be accounted among Herschel's highest claims to glory, even after the mention that my duty as historian has obliged me to make of the anterior conjectures by Fontenelle, by Bradley, by Mayer, and by Lambert.

By the side of this great discovery we should place another, that seems likely to expand in future. The results which it allows us to hope for will be of extreme importance. The discovery here alluded to was announced to the learned world in 1803: it is that of the reciprocal dependence of several stars, connected the one with the other, as the several planets and their satellites of our system are with the sun.

Let us to these immortal labours add the ingenious ideas that we owe to Herschel on the nebulæ, on the constitution of the Milky-way, on the universe as a whole: ideas which almost by

themselves constitute the actual history of the formation of the worlds, and we cannot but have a deep reverence for that powerful genius that has scarcely ever erred, notwithstanding an ardent imagination.

LABOURS RELATIVE TO THE SOLAR SYSTEM.

Herschel occupied himself very much with the sun, but only relative to its physical constitution. The observations that the illustrious astronomer made on this subject, the consequences that he deduced from them, equal the most ingenious discoveries for which the sciences are indebted to him.

In his important memoir in 1795, the great astronomer declares himself convinced that the substance by the intermediation of which the sun shines, cannot be either a liquid, or an elastic fluid. It must be analogous to our clouds, and float in the transparent atmosphere of that body. The sun has, according to him, two atmospheres, endowed with motions quite independent of each other. An elastic fluid of an unknown nature is being constantly formed on the dark surface of the sun, and rising up on account of its specific lightness, it forms the *pores* in the stratum of reflecting clouds; then, combining with other gases, it produces the wrinkles in the region of luminous clouds. When the ascending currents are powerful, they give rise to the *nuclei*, to the *penumbræ*, to the *faculæ*. If this explanation of the formation of solar spots is well founded, we must expect to find that the sun does not constantly emit similar quantities of light and heat. Recent observations have verified this conclusion. But large nuclei, large penumbræ, wrinkles, faculæ, do they indicate an abundant luminous and calorific emission, as Herschel thought; that would be the result of his hypothesis on the existence of very active ascending currents, but direct experience seems to contradict it.

The following is the way in which a learned man, Sir David Brewster, appreciates this view of Herschel's: "It is not conceivable that luminous clouds, ceding to the lightest impulses and in a state of constant change, can be the source of the sun's devouring flame and of the dazzling light which it emits; nor can we admit besides, that the feeble barrier formed by planetary clouds would shelter the objects that it might cover, from the destructive effects of the superior elements."

Sir D. Brewster imagines that the non-luminous rays of caloric, which form a constituent part of the solar light, are emitted by the dark nucleus of the sun; whilst the visible coloured rays proceed from the luminous matter by which the nucleus is surrounded. "From thence," he says, "proceeds the reason of light and heat always appearing in a state of combination: the one

emanation cannot be obtained without the other. With this hypothesis we should explain naturally why it is hottest when there are most spots, because the heat of the nucleus would then reach us without having been weakened by the atmosphere that it usually has to traverse." But it is far from being an ascertained fact, that we experience increased heat during the apparition of solar spots; the inverse phenomenon is more probably true.

Herschel occupied himself also with the physical constitution of the moon. In 1780, he sought to measure the height of our satellite's mountains. The conclusion that he drew from his observations was, that few of the lunar mountains exceed 800 metres (or 2600 feet). More recent selenographic studies differ from this conclusion. There is reason to observe on this occasion how much the result surmised by Herschel differs from any tendency to the extraordinary or the gigantic, that has been so unjustly assigned as the characteristic of the illustrious astronomer.

At the close of 1787, Herschel presented a memoir to the Royal Society, the title of which must have made a strong impression on people's imaginations. The author therein relates that on the 19th of April, 1787, he had observed in the non-illuminated part of the moon, that is, in the then dark portion, three volcanoes in a state of ignition. Two of those volcanoes appeared to be on the decline, the other appeared to be active. Such was then Herschel's conviction of the reality of the phenomenon, that the next morning he wrote thus of his first observation: "The volcano burns with more violence than last night." The real diameter of the volcanic light was 5000 metres (16,400 English feet). Its intensity appeared very superior to that of the nucleus of a comet then in apparition. The observer added: "The objects situated near the crater are feebly illuminated by the light that emanates from it." Herschel concludes thus: "In short, this eruption very much resembles the one I witnessed on the 4th of May, 1783."

How happens it, after such exact observations, that few astronomers now admit the existence of active volcanoes in the moon? I will explain this singularity in a few words.

The various parts of our satellite are not all equally reflecting. Here, it may depend on the form, elsewhere, on the nature of the materials. Those persons who have examined the moon with telescopes, know how very considerable the difference arising from these two causes may be, how much brighter one point of the moon sometimes is than those around it. Now, it is quite evident that the relations of intensity between the faint parts and the brilliant parts must continue to exist, whatever be the origin of the illuminating light. In the portion of the lunar globe that is illuminated by the sun, there are, everybody knows, some

points, the brightness of which is extraordinary compared to those around them; those same points, when they are seen in that portion of the moon that is only lighted by the earth, or in the ash-coloured part, will still predominate over the neighbouring regions by their comparative intensity. Thus we may explain the observations of the Slough astronomer, without recurring to volcanoes. Whilst the great observer was studying in the non-illuminated portion of the moon, the supposed volcano of the 20th of April, 1787, his nine-foot telescope showed him in truth, by the aid of the secondary rays proceeding from the earth, even the darkest spots.

Herschel did not recur to the discussion of the supposed actually burning lunar volcanoes, until 1791. In the volume of the *Philosophical Transactions* for 1792, he relates that in directing a twenty-foot telescope, magnifying 360 times, to the entirely eclipsed moon on the 22nd of October, 1790, there were visible, over the whole face of the satellite, about a hundred and fifty very luminous red points. The author declares that he will observe the greatest reserve relative to the similarity of all these points, their great brightness, and their remarkable colour.

Yet is not red the usual colour of the moon when eclipsed, and when it has not entirely disappeared? Could the solar rays reaching our satellite by the effect of refraction, and after an absorption experienced in the lowest strata of the terrestrial atmosphere, receive another tint? Are there not in the moon, when freely illuminated, and opposite to the sun, from one to two hundred little points, remarkable by the brightness of their light? Would it be possible for those little points not to be also distinguishable in the moon, when it receives only the portion of solar light which is refracted and coloured by our atmosphere?

Herschel was more successful in his remarks on the absence of a lunar atmosphere. During the solar eclipse of the 5th September, 1793, the illustrious astronomer particularly directed his attention to the shape of the acute horn resulting from the intersection of the limbs of the moon and of the sun. He deduced from his observation that if towards the point of the horn there had been a deviation of only one second, occasioned by the refraction of the solar light in the lunar atmosphere, it would not have escaped him.

Herschel made the planets the object of numerous researches. Mercury was the one with which he least occupied himself; he found its disc perfectly round on observing it during its projection, that is to say, in astronomical language, during its transit over the sun on the 9th of November, 1802. He sought to determine the time of the rotation of Venus since the year 1777.

He published two memoirs relative to Mars, the one in 1781, the other in 1784, and the discovery of its being flattened at the poles we owe to him. After the discovery of the small planets, Ceres, Pallas, Juno, and Vesta, by Piazzi, Olbers, and Harding, Herschel applied himself to measuring their angular diameter. He concluded from his researches that those four new bodies did not deserve the name of planets, and he proposed to call them asteroïds. This epithet was subsequently adopted; though bitterly criticised by a historian of the Royal Society of London, Dr. Thomson, who went so far as to suppose that the learned astronomer "had wished to deprive the first observers of those bodies, of all idea of rating themselves as high as him (Herschel) in the scale of astronomical discoverers." I should require nothing farther to annihilate such an imputation, than to put it by the side of the following passage, extracted from a memoir by this celebrated astronomer published in the *Philosophical Transactions*, for the year 1805: "The specific difference existing between planets and asteroïds appears now, by the addition of a third individual of the latter species, to be more completely established, and that circumstance, in my opinion, has added more to the *ornament* of our system than the discovery of a new planet could have done."

Although much has not resulted from Herschel's having occupied himself with the physical constitution of Jupiter, astronomy is indebted to him for several important results relative to the duration of that planet's rotation. He also made numerous observations on the intensities and comparative magnitudes of its satellites.

The compression of Saturn, the duration of its rotation, the physical constitution of this planet and that of its ring, were, on the part of Herschel, the object of numerous researches which have much contributed to the progress of planetary astronomy. But on this subject two important discoveries especially added new glory to the great astronomer.

Of the five known satellites of Saturn at the close of the 17th century, Huygens had discovered the fourth; Cassini the others.

The subject seemed to be exhausted, when news from Slough showed what a mistake this was.

On the 28th of August, 1789, the great forty-foot telescope revealed to Herschel a satellite still nearer to the ring than the other five already observed. According to the principles of the nomenclature previously adopted, the small body of the 28th August ought to have been called the first satellite of Saturn, the numbers indicating the places of the other five would then have been each increased by a unity. But the fear of introducing

confusion into science by these continual changes of denomination, induced a preference for calling the new satellite the sixth.

Thanks to the prodigious powers of the forty-foot telescope, a last satellite, the seventh, showed itself on the 17th of September, 1789, between the sixth and the ring.

This seventh satellite is extremely faint. Herschel, however, succeeded in seeing it whenever circumstances were very favourable, even by the aid of the twenty-foot telescope.

The discovery of the planet Uranus, the detection of its satellites, will always occupy one of the highest places among those by which modern astronomy is honoured.

On the 13th of March, 1781, between ten and eleven o'clock at night, Herschel was examining the small stars near H Geminorum with a seven-foot telescope, bearing a magnifying power of 227 times. One of these stars seemed to him to have an unusual diameter. The celebrated astronomer, therefore, thought it was a comet. It was under this denomination that it was then discussed at the Royal Society of London. But the researches of Herschel and of Laplace showed later that the orbit of the new body was nearly circular, and Uranus was elevated to the rank of a planet.

The immense distance of Uranus, its small angular diameter, the feebleness of its light, did not allow the hope, that if that body had satellites, the magnitudes of which were, relatively to its own size, what the satellites of Jupiter, of Saturn are, compared to those two large planets, any observer could perceive them from the earth. Herschel was not a man to be deterred by such discouraging conjectures. Therefore, since powerful telescopes of the ordinary construction, that is to say, with two mirrors conjugated, had not enabled him to discover anything, he substituted, in the beginning of January 1787, *front view* telescopes, that is, telescopes throwing much more light on the objects, the small mirror being then suppressed, and with it one of the causes of loss of light is got rid of.

By patient labour, by observations requiring a rare perseverance, Herschel attained (from the 11th of January 1787 to the 28th of February 1794), to the discovery of the six satellites of his planet, and thus to complete the *world* of a system that belongs entirely to himself.

There are several of Herschel's memoirs on comets. In analysing them, we shall see that this great observer could not touch anything without making further discoveries in the subject.

Herschel applied some of his fine instruments to the study of the physical constitution of a comet discovered by Mr. Pigott, on the 28th September, 1807.

The nucleus was round and well determined. Some measures

taken on the day when the nucleus subtended only an angle of a single second, gave as its real angle $\frac{6}{100}$ of the diameter of the earth.

Herschel saw no phase at an epoch when only $\frac{7}{10}$ of the nucleus could be illuminated by the sun. The nucleus then must shine by its own light.

This is a legitimate inference in the opinion of every one who will allow, on one hand, that the nucleus is a solid body, and on the other, that it would have been possible to observe a phase of $\frac{8}{10}$ on a disk whose apparent total diameter did not exceed one or two seconds of a degree.

Very small stars seemed to grow much paler when they were seen through the coma or through the tail of the comet.

This faintness may have only been apparent, and might arise from the circumstance of the stars being then projected on a luminous background. Such is, indeed, the explanation adopted by Herschel. A gaseous medium capable of reflecting sufficient solar light to efface that of some stars, would appear to him to possess in each stratum a sensible quantity of matter, and to be, for that reason, a cause of real diminution of the light transmitted, though nothing reveals the existence of such a cause.

This argument, offered by Herschel in favour of the system which transforms comets into self-luminous bodies, has not, as we may perceive, much force. I might venture to say as much of many other remarks by this great observer. He tells us that the comet was very visible in the telescope on the 21st of February 1808; now, on that day, its distance from the sun amounted to 2·7 times the mean radius of the terrestrial orbit; its distance from the observer was 2·9: "What probability would there be that rays going to such distances, from the sun to the comet, could, after their reflection, be seen by an eye nearly three times more distant from the comet than from the sun?"

It is only numerical determinations that could give value to such an argument. By satisfying himself with vague reasoning, Herschel did not even perceive that he was committing a great mistake by making the comet's distance from the observer appear to be an element of visibility. If the comet be self-luminous, its intrinsic splendour (its brightness for unity of surface) will remain constant at any distance, as long as the subtended angle remains sensible. If the body shines by borrowed light, its brightness will vary only according to its change of distance from the sun; nor will the distance of the observer occasion any change in the visibility; always, let it be understood, with the restriction that the apparent diameter shall not be diminished below certain limits.

Herschel finished his observations of a comet that was visible in January 1807, with the following remark: —

"Of the sixteen telescopic comets that I have examined, fourteen had no solid body visible at their centre; the other two exhibited a central light, very ill defined, that might be termed a nucleus, but a light that certainly could not deserve the name of a disk."

The beautiful comet of 1811 became the object of that celebrated astronomer's conscientious labour. Large telescopes showed him, in the midst of the gazeous head, a rather reddish body of planetary appearance, which bore strong magnifying powers, and showed no sign of phase. Hence Herschel concluded that it was self-luminous. Yet if we reflect that the planetary body under consideration was not a second in diameter, the absence of a phase does not appear a demonstrative argument.

The light of the head had a blueish-green tint. Was this a real tint, or did the central reddish body, only through contrast, make the surrounding vapour appear to be coloured? Herschel did not examine the question in this point of view.

The head of the comet appeared to be enveloped at a certain distance, on the side towards the sun, by a brilliant narrow zone, embracing about a semicircle, and of a yellowish colour. From the two extremities of the semicircle there arose, towards the region away from the sun, two long luminous streaks which limited the tail. Between the brilliant circular semi-ring and the head, the cometary substance seemed dark, very rare, and very diaphanous.

The luminous semi-ring always presented similar appearances in all the positions of the comet; it was not then possible to attribute to it really the annular form, the shape of Saturn's ring, for example. Herschel sought whether a spherical demi-envelope of luminous matter, and yet diaphanous, would not lead to a natural explanation of the phenomenon. In this hypothesis, the visual rays, which on the 6th of October, 1811, made a section of the envelope, or bore almost tangentially, traversed a thickness of matter of about 399,000 kilometres (248,000 English miles), whilst the visual rays near the head of the comet did not meet above 80,000 kilometres (50,000 miles) of it. As the brightness must be proportional to the quantity of matter traversed, there could not fail to be an appearance around the comet, of a semi-ring five times more luminous than the central regions. This semi-ring, then, was an effect of projection, and it has revealed a circumstance to us truly remarkable in the physical constitution of comets.

The two luminous streaks that outlined the tail at its two limits,

may be explained in a similar manner; the tail was not flat as it appeared to be; it had the form of a conoid, with its sides of a certain thickness. The visual lines which traversed those sides almost tangentially, evidently met much more matter than the visual lines passing across. This maximum of matter could not fail of being represented by a maximum of light.

The luminous semi-ring floated; it appeared one day to be suspended in the diaphanous atmosphere by which the head of the comet was surrounded, at a distance of 518,000 kilometres (322,000 English miles) from the nucleus.

This distance was not constant. The matter of the semi-annular envelope seemed even to be precipitated by slow degrees through the diaphanous atmosphere; finally it reached the nucleus; the earlier appearances vanished; the comet was reduced to a globular nebula.

During its period of dissolution, the ring appeared sometimes to have several branches.

The luminous shreds of the tail seemed to undergo rapid, frequent, and considerable variations of length. Herschel discerned symptoms of a movement of rotation both in the comet and in its tail. This rotatory motion carried unequal shreds from the centre towards the border, and reciprocally. On looking from time to time at the same region of the tail, at the border for example, sensible changes of length must have been perceptible, which however had no reality in them. Herschel thought, as I have already said, that the beautiful comet of 1811 and that of 1807 were self-luminous. The second comet of 1811 appeared to him to shine only by borrowed light. It must be acknowledged that these conjectures did not rest on anything demonstrative.

In attentively comparing the comet of 1807 with the beautiful comet of 1811, relative to the changes of distance from the sun, and the modifications resulting thence, Herschel put it beyond doubt that these modifications have something individual in them, something relative to a special state of the nebulous matter. On one celestial body the changes of distance produce an enormous effect, on another the modifications are insignificant.

OPTICAL LABOURS.

I shall say very little on the discoveries that Herschel made in physics. In short, everybody knows them. They have been inserted into special treatises, into elementary works, into verbal instruction; they must be considered as the starting point of a multitude of important labours with which the sciences have been enriched during several years.

The chief of these is that of the dark radiating heat which is found mixed with light.

In studying the phenomena, no longer with the eye, like Newton, but with a thermometer, Herschel discovered that the solar spectrum is prolonged on the red side far beyond the visible limits. The thermometer sometimes rose higher in that dark region, than in the midst of brilliant zones. The light of the sun then, contains, besides the coloured rays so well characterised by Newton, some invisible rays, still less refrangible than the red, and whose warming power is very considerable. A world of discoveries has arisen from this fundamental fact.

The dark heat emanating from terrestrial objects more or less heated, became also subjects of Herschel's investigations. His work contained the germs of a good number of beautiful experiments since erected upon it in our own day.

By successively placing the same objects in all parts of the solar spectrum, Herschel determined the illuminating powers of the various prismatic rays. The general result of these experiments may be thus enunciated:

The illuminating power of the red rays is not very great; that of the orange rays surpasses it, and is in its turn surpassed by the power of the yellow rays. The maximum power of illumination is found between the brightest yellow and the palest green. The yellow and the green possess this power equally. A like assimilation may be laid down between the blue and the red. Finally, the power of illumination in the indigo rays, and above all in the violet, is very weak.

Yet the memoirs of Herschel on Newton's coloured rings, though containing a multitude of exact experiments, have not much contributed to advance the theory of those curious phenomena. I have learnt from good authority, that the great astronomer held the same opinion on this topic. He said that it was the only occasion on which he had reason to regret having, according to his constant method, published his labours immediately, as fast as they were performed.

LAPLACE.

Having been appointed to draw up the report of a committee of the Chamber of Deputies which was nominated in 1842, for the purpose of taking into consideration the expediency of a proposal submitted to the Chamber by the Minister of Public Instruction, relative to the publication of a new edition of the works of Laplace at the public expense, I deemed it to be my duty to embody in the report a concise analysis of the works of our illustrious countryman. Several persons, influenced, perhaps, by too indulgent a feeling towards me, having expressed a wish that this analysis should not remain buried amid a heap of legislative documents, but that it should be published in the *Annuaire du Bureau des Longitudes*, I took advantage of this circumstance to develope it more fully so as to render it less unworthy of public attention. The scientific part of the report presented to the Chamber of Deputies will be found here entire. It has been considered desirable to suppress the remainder. I shall merely retain a few sentences containing an explanation of the object of the proposed law, and an announcement of the resolutions which were adopted by the three powers of the State.

"Laplace has endowed France, Europe, the scientific world, with three magnificent compositions: the *Traité de Mécanique Céleste*, the *Exposition du Système du Monde*, and the *Théorie Analytique des Probabilités*. In the present day (1842) there is no longer to be found a single copy of this last work at any bookseller's establishment in Paris. The edition of the *Mécanique Céleste* itself will soon be exhausted. It was painful then to reflect that the time was close at hand when persons engaged in the study of the higher mathematics would be compelled, for want of the original work, to inquire at Philadelphia, at New York, or at Boston for the English translation of the *chef d'œuvre* of our countryman by the excellent geometer Bowditch. These fears, let us hasten to state, were not well founded. To republish the *Mécanique Céleste* was, on the part of the family of the illustrious geometer, to perform a pious duty. Accordingly, Madame de Laplace, who is so justly, so profoundly attentive to every circum-

stance calculated to enhance the renown of the name which she bears, did not hesitate about pecuniary considerations. A small property near Pont l'Evêque was about to change hands, and the proceeds were to have been applied so that Frenchmen should not be deprived of the satisfaction of exploring the treasures of the *Mécanique Céleste* through the medium of the vernacular tongue.

"The republication of the complete works of Laplace rested upon an equally sure guarantee. Yielding at once to filial affection, to a noble feeling of patriotism, and to the enthusiasm for brilliant discoveries which a course of severe study inspired, General Laplace had long since qualified himself for becoming the editor of the seven volumes which are destined to immortalise his father.

"There are glorious achievements of a character too elevated, of a lustre too splendid, that they should continue to exist as objects of private property. Upon the State devolves the duty of preserving them from indifference and oblivion; of continually holding them up to attention, of diffusing a knowledge of them through a thousand channels; in a word, of rendering them subservient to the public interests.

"Doubtless the Minister of Public Instruction was influenced by these considerations, when upon the occasion of a new edition of the works of Laplace having become necessary, he demanded of you to substitute the great French family for the personal family of the illustrious geometer. We give our full and unreserved adhesion to this proposition. It springs from a feeling of patriotism which will not be gainsayed by any one in this assembly."

In fact, the Chamber of Deputies had only to examine and solve this single question: "Are the works of Laplace of such transcendent, such exceptional merit, that their republication ought to form the subject of deliberation of the great powers of the State?" An opinion prevailed, that it was not enough merely to appeal to public notoriety, but that it was necessary to give an exact analysis of the brilliant discoveries of Laplace in order to exhibit more fully the importance of the resolution about to be adopted. Who could hereafter propose on any similar occasion that the Chamber should declare itself without discussion, when a desire was felt, previous to voting in favour of a resolution so honourable to the memory of a great man, to fathom, to measure, to examine minutely and from every point of view monuments such as the *Mécanique Céleste* and the *Exposition du Système du Monde?* It has appeared to me that the report drawn up in the name of a committee of one of the three great powers of the State might worthily close this series of biographical notices of eminent astronomers.*

* The author here refers to the series of biographies contained in tome III. of the *Notices Biographiques.* — *Translator.*

The Marquis de Laplace, peer of France, one of the forty of the French Academy, member of the Academy of Sciences and of the *Bureau des Longitudes,* an associate of all the great Academies or Scientific Societies of Europe, was born at Beaumont-en-Auge of parents belonging to the class of small farmers, on the 28th of March 1749; he died on the 5th of March 1827.

The first and second volumes of the *Mécanique Céleste* were published in 1799; the third volume appeared in 1802, the fourth volume in 1805; as regards the fifth volume, Books XI. and XII. were published in 1823, Books XIII. XIV. and XV. in 1824, and Book XVI. in 1825. The *Théorie des Probabilités* was published in 1812. We shall now present the reader with the history of the principal astronomical discoveries contained in these immortal works.

Astronomy is the science of which the human mind may most justly boast. It owes this indisputable pre-eminence to the elevated nature of its object, to the grandeur of its means of investigation, to the certainty, the utility, and the unparalleled magnificence of its results.

From the earliest period of the social existence of mankind, the study of the movements of the heavenly bodies has attracted the attention of governments and peoples. To several great captains, illustrious statesmen, philosophers, and eminent orators of Greece and Rome it formed a subject of delight. Yet, let us be permitted to state, astronomy truly worthy of the name is quite a modern science. It dates only from the sixteenth century.

Three great, three brilliant phases, have marked its progress.

In 1543 Copernicus overthrew with a firm and bold hand, the greater part of the antique and venerable scaffolding with which the illusions of the senses and the pride of successive generations had filled the universe. The earth ceased to be the centre, the pivot of the celestial movements; it henceforward modestly ranged itself among the planets; its material importance, amid the totality of the bodies of which our solar system is composed, found itself reduced almost to that of a grain of sand.

Twenty-eight years had elapsed from the day when the Canon of Thorn expired while holding in his faltering hands the first copy of the work which was to diffuse so bright and pure a flood of glory upon Poland, when Würtemberg witnessed the birth of a man who was destined to achieve a revolution in science not less fertile in consequences, and still more difficult of execution. This man was Kepler. Endowed with two qualities which seemed incompatible with each other, a volcanic imagination, and a pertinacity of intellect which the most tedious numerical calculations could not daunt, Kepler conjectured that the movements of the celestial bodies must be connected together by simple laws, or, to

use his own expressions, by *harmonic* laws. These laws he undertook to discover. A thousand fruitless attempts, errors of calculation inseparable from a colossal undertaking, did not prevent him a single instant from advancing resolutely towards the goal of which he imagined he had obtained a glimpse. Twenty-two years were employed by him in this investigation and still he was not weary of it! What, in reality, are twenty-two years of labour to him who is about to become the legislator of worlds; who shall inscribe his name in ineffaceable characters upon the frontispiece of an immortal code; who shall be able to exclaim in dithyrambic language, and without incurring the reproach of any one, "The die is cast; I have written my book; it will be read either in the present age or by posterity, it matters not which; it may well await a reader since God has waited six thousand years for an interpreter of his works?" *

To investigate a physical cause capable of making the planets revolve in closed curves; to place the principle of the stability of the universe in mechanical forces and not in solid supports such as the spheres of crystal which our ancestors had dreamed of; to extend to the revolutions of the heavenly bodies the general principles of the mechanics of terrestrial bodies,—such were the questions which remained to be solved after Kepler had announced his discoveries to the world.

Very distinct traces of these great problems are perceived here and there among the ancients as well as the moderns, from Lucretius and Plutarch down to Kepler, Bouillaud, and Borelli. It is to Newton, however, that we must award the merit of their solution. This great man, like several of his predecessors, conceived the celestial bodies to have a tendency to approach towards each other in virtue of an attractive force, deduced the mathematical characteristics of this force from the laws of Kepler, extended it to all the material molecules of the solar system, and developed his brilliant discovery in a work which, even in the present day, is regarded as the most eminent production of the human intellect.

* These celebrated laws, known in astronomy as the laws of Kepler, are three in number. The first law is, that the planets describe ellipses around the sun in their common focus; the second, that a line joining the planet and the sun sweeps over equal areas in equal times; the third, that the squares of the periodic times of the planets are proportional to the cubes of their mean distances from the sun. The first two laws were discovered by Kepler in the course of a laborious examination of the theory of the planet Mars; a full account of this inquiry is contained in his famous work *De Stella Martis*, published in 1609. The discovery of the third law was not effected until several years afterwards, Kepler announced it to the world in his treatise on Harmonics (1628.). The passage quoted below is extracted from that work. — *Translator.*

The heart aches when, upon studying the history of the sciences, we perceive so magnificent an intellectual movement effected without the co-operation of France. Practical astronomy increased our inferiority. The means of investigation were at first inconsiderately entrusted to foreigners, to the prejudice of Frenchmen abounding in intelligence and zeal. Subsequently intellects of a superior order struggled with courage, but in vain, against the unskilfulness of our artists. During this period, Bradley more fortunate on the other side of the Channel immortalised himself by the discovery of aberration and nutation.

The contribution of France to these admirable revolutions in astronomical science, consisted, in 1740, of the experimental determination of the spheroidal figure of the earth, and of the discovery of the variation of gravity upon the surface of our planet. These were two great results; our country, however, had a right to demand more: when France is not in the first rank she has lost her place.*

This rank, which was lost for a moment, was brilliantly regained, an achievement for which we are indebted to four geometers.

When Newton, giving to his discoveries a generality which the laws of Kepler did not imply, imagined that the different planets were not only attracted by the sun, but that they also attract each other, he introduced into the heavens a cause of universal disturbance. Astronomers could then see at the first glance that in no part of the universe whether near or distant would the Keplerian laws suffice for the exact representation of the phenomena; that the simple, regular movements with which the imaginations of the ancients were pleased to endue the heavenly bodies would experience numerous, considerable, perpetually changing perturbations.

To discover several of these perturbations, to assign their nature,

* The spheroidal figure of the earth was established by the comparison of an arc of the meridian that had been measured in France, with a similar arc measured in Lapland, from which it appeared that the length of a degree of the meridian increases from the equator towards the poles, conformably to what ought to result upon the supposition of the earth having the figure of an oblate spheroid. The length of the Lapland arc was determined by means of an expedition which the French Government had despatched to the North of Europe for that purpose. A similar expedition had been despatched from France about the same time to Peru in South America, for the purpose of measuring an arc of the meridian under the equator, but the results had not been ascertained at the time to which the author alludes in the text. The variation of gravity at the surface of the earth was established by Richer's experiments with the pendulum at Cayenne, in South America (1673-4), from which it appeared that the pendulum oscillates more slowly — and consequently the force of gravity is less intense — under the equator than in the latitude of Paris. — *Translator.*

and in a few rare cases their numerical values, such was the object which Newton proposed to himself in writing the *Principia Mathematica Philosophiæ Naturalis.*

Notwithstanding the incomparable sagacity of its author the Principia contained merely a rough outline of the planetary perturbations. If this sublime sketch did not become a complete portrait we must not attribute the circumstance to any want of ardour or perseverance; the efforts of the great philosopher were always superhuman, the questions which he did not solve were incapable of solution in his time. When the mathematicians of the continent entered upon the same career, when they wished to establish the Newtonian system upon an incontrovertible basis, and to improve the tables of astronomy, they actually found in their way difficulties which the genius of Newton had failed to surmount.

Five geometers, Clairaut, Euler, D'Alembert, Lagrange, and Laplace, shared between them the world of which Newton had disclosed the existence. They explored it in all directions, penetrated into regions which had been supposed inaccessible, pointed out there a multitude of phenomena which observation had not yet detected; finally, and it is this which constitutes their imperishable glory, they reduced under the domain of a single principle, a single law, everything that was most refined and mysterious in the celestial movements. Geometry had thus the boldness to dispose of the future; the evolutions of ages are scrupulously ratifying the decisions of science.

We shall not occupy our attention with the magnificent labours of Euler, we shall, on the contrary, present the reader with a rapid analysis of the discoveries of his four rivals, our countrymen.*

If a celestial body, the moon for example, gravitated solely towards the centre of the earth, it would describe a mathematical ellipse; it would strictly obey the laws of Kepler, or, which is the same thing, the principles of mechanics expounded by Newton in the first sections of his immortal work.

Let us now consider the action of a second force. Let us take into account the attraction which the sun exercises upon the moon, in other words, instead of two bodies let us suppose three to

* It may perhaps be asked why we place Lagrange among the French geometers? This is our reply: It appears to us that the individual who was named Lagrange Tournier, two of the most characteristic French names which it is possible to imagine, whose maternal grandfather was M. Gros, whose paternal great grandfather was a French officer a native of Paris, who never wrote except in French, and who was invested in our country with high honours during a period of nearly thirty years; — ought to be regarded as a Frenchman although born at Turin. — *Author.*

operate on each other, the Keplerian ellipse will now furnish merely a rough indication of the motion of our satellite. In some parts the attraction of the sun will tend to enlarge the orbit, and will in reality do so; in other parts the effect will be the reverse of this. In a word, by the introduction of a third attractive body, the greatest complication will succeed to a simple regular movement upon which the mind reposed with complacency.

If Newton gave a complete solution of the question of the celestial movements in the case wherein two bodies attract each other, he did not even attempt an analytical investigation of the infinitely more difficult problem of three bodies. The problem of three bodies (this is the name by which it has become celebrated), the problem for determining the movement of a body subjected to the attractive influence of two other bodies, was solved for the first time, by our countryman Clairaut.* From this solution we may date the important improvements of the lunar tables effected in the last century.

The most beautiful astronomical discovery of antiquity, is that of the precession of the equinoxes. Hipparchus, to whom the honour of it is due, gave a complete and precise statement of all the consequences which flow from this movement. Two of these have more especially attracted attention.

By reason of the precession of the equinoxes, it is not always the same groups of stars, the same constellations, which are perceived in the heavens at the same season of the year. In the lapse of ages the constellations of winter will become those of summer and reciprocally.

By reason of the precession of the equinoxes, the pole does not always occupy the same place in the starry vault. The moderately bright star which is very justly named in the present day, the pole star, was far removed from the pole in the time of Hipparchus; in the course of a few centuries it will again appear removed from it. The designation of pole star has been, and will be, applied to stars very distant from each other.

When the inquirer in attempting to explain natural phenomena has the misfortune to enter upon a wrong path, each precise observation throws him into new complications. Seven spheres of crystal did not suffice for representing the phenomena as soon as the illustrious astronomer of Rhodes discovered precession. An eighth

* The problem of three bodies was solved independently about the same time by Euler, D'Alembert, and Clairaut. The two last-mentioned geometers communicated their solutions to the Academy of Sciences on the same day, November 15, 1747. Euler had already in 1746 published tables of the moon, founded on his solution of the same problem, the details of which he subsequently published in 1753. — *Translator.*

phere was then wanted to account for a movement in which all he stars participated at the same time.

Copernicus having deprived the earth of its alleged immobility, gave a very simple explanation of the most minute circumstances of precession. He supposed that the axis of rotation does not remain exactly parallel to itself; that in the course of each complete revolution of the earth around the sun, the axis deviates from its position by a small quantity; in a word, instead of supposing the circumpolar stars to advance in a certain way towards the pole, he makes the pole advance towards the stars. This hypothesis divested the mechanism of the universe of the greatest complication which the love of theorising had introduced into it. A new Alphonse would have then wanted a pretext to address to his astronomical synod the profound remark, so erroneously interpreted, which history ascribes to the king of Castile.

If the conception of Copernicus improved by Kepler had, as we have just seen, introduced a striking improvement into the mechanism of the heavens, it still remained to discover the motive force which, by altering the position of the terrestrial axis during each successive year, would cause it to describe an entire circle of nearly 50° in diameter, in a period of about 26,000 years.

Newton conjectured that this force arose from the action of the sun and moon upon the redundant matter accumulated in the equatorial regions of the earth: thus he made the precession of the equinoxes depend upon the spheroidal figure of the earth; he declared that upon a round planet no precession would exist.

All this was quite true, but Newton did not succeed in establishing it by a mathematical process. Now this great man had introduced into philosophy the severe and just rule: Consider as certain only what has been demonstrated. The demonstration of the Newtonian conception of the precession of the equinoxes was, then, a great discovery, and it is to D'Alembert that the glory of it is due.* The illustrious geometer gave a complete explanation of the general movement, in virtue of which the terrestrial axis returns to the same stars in a period of about 26,000 years. He

* It must be admitted that M. Arago has here imperfectly represented Newton's labours on the great problem of the precession of the equinoxes. The immortal author of the Principia did not merely *conjecture* that the conical motion of the earth's axis is due to the disturbing action of the sun and moon upon the matter accumulated around the earth's equator: he *demonstrated* by a very beautiful and satisfactory process that the movement must necessarily arise from that cause; and although the means of investigation, in his time, were inadequate to a rigorous computation of the quantitative effect, still his researches on the subject have been always regarded as affording one of the most striking proofs of sagacity which is to be found in all his works.—*Translator*.

also connected with the theory of gravitation the perturbation of precession discovered by Bradley, that remarkable oscillation which the earth's axis experiences continually during its movement of progression, and the period of which, amounting to about eighteen years, is exactly equal to the time which the intersection of the moon's orbit with the ecliptic employs in describing the 360° of the entire circumference.

Geometers and astronomers are justly occupied as much with the figure and physical constitution which the earth might have had in remote ages as with its present figure and constitution.

As soon as our countryman Richer discovered that a body, whatever be its nature, weighs less when it is transported nearer the equatorial regions, everybody perceived that the earth, if it was originally fluid, ought to bulge out at the equator. Huyghens and Newton did more; they calculated the difference between the greatest and least axes, the excess of the equatorial diameter over the line of the poles.*

The calculation of Huyghens was founded upon hypothetic properties of the attractive force which were wholly inadmis-

* It would appear that Hooke had conjectured that the figure of the earth might be spheroidal before Newton or Huyghens turned their attention to the subject. At a meeting of the Royal Society on the 28th of February, 1678, a discussion arose respecting the figure of Mercury which M. Gallet of Avignon had remarked to be oval on the occasion of the planet's transit across the sun's disk on the 7th of November, 1677. Hooke was inclined to suppose that the phenomenon was real, and that it was due to the whirling of the planet on an axis "which made it somewhat of the shape of a turnip, or of a solid made by an ellipsis turned round upon its shorter diameter." At the meeting of the Society on the 7th of March, the subject was again discussed. In reply to the objection offered to his hypothesis on the ground of the planet being a solid body, Hooke remarked that "although it might now be solid, yet that at the beginning it might have been fluid enough to receive that shape; and that although this supposition should not be granted, it would be probable enough that it would really run into that shape and make the same appearance; *and that it is not improbable but that the water here upon the earth might do it in some measure by the influence of the diurnal motion, which, compounded with that of the moon, he conceived to be the cause of the Tides.*" (Journal Book of the Royal Society, vol. vi. p. 60.) Richer returned from Cayenne in the year 1674, but the account of his observations with the pendulum during his residence there, was not published until 1679, nor is there to be found any allusion to them during the intermediate interval, either in the volumes of the Academy of Sciences or any other publication. We have no means of ascertaining how Newton was first induced to suppose that the figure of the earth is spheroidal, but we know, upon his own authority, that as early as the year 1667, or 1668, he was led to consider the effects of the centrifugal force in diminishing the weight of bodies at the equator. With respect to Huyghens, he appears to have formed a conjecture respecting the spheroidal figure of the earth independently of Newton; but his method for computing the ellipticity is founded upon that given in the Principia. — *Translator.*

sible; that of Newton upon a theorem which he ought to have demonstrated; the theory of the latter was characterised by a defect of a still more serious nature: it supposed the density of the earth during the original state of fluidity, to be homogeneous.* When in attempting the solution of great problems we have recourse to such simplifications; when, in order to elude difficulties of calculation, we depart so widely from natural and physical conditions, the results relate to an ideal world, they are in reality nothing more than flights of the imagination.

In order to apply mathematical analysis usefully to the determination of the figure of the earth it was necessary to abandon all idea of homogeneity, all constrained resemblance between the forms of the superposed and unequally dense strata; it was necessary also to examine the case of a central solid nucleus. This generality increased tenfold the difficulties of the problem; neither Clairaut nor D'Alembert was, however, arrested by them. Thanks to the efforts of these two eminent geometers, thanks to some essential developements due to their immediate successors, and especially to the illustrious Legendre, the theoretical determination of the figure of the earth has attained all desirable perfection. There now reigns the most satisfactory accordance between the results of calculation and those of direct measurement. The earth, then, was originally fluid: analysis has enabled us to ascend to the earliest ages of our planet.†

In the time of Alexander comets were supposed by the majority of the Greek philosophers to be merely meteors generated in our atmosphere. During the middle ages, persons, without giving themselves much concern about the nature of those bodies, supposed them to prognosticate sinister events. Regiomontanus and Tycho Brahé proved by their observations that they are situate beyond the moon; Hevelius, Dörfel, &c., made them revolve

* Newton assumed that a homogeneous fluid mass of a spheroidal form would be in equilibruim if it were endued with an adequate rotatory motion and its constituent particles attracted each other in the inverse proportion of the square of the distance. Maclaurin first demonstrated the truth of this theorem by a rigorous application of the ancient geometry. — *Translator.*

† The results of Clairaut's researches on the figure of the earth are mainly embodied in a remarkable theorem discovered by that geometer, and which may be enunciated thus : — *The sum of the fractions expressing the ellipticity and the increase of gravity at the pole is equal to two and a half times the fraction expressing the centrifugal force at the equator, the unit of force being represented by the force of gravity at the equator.* This theorem is independent of any hypothesis with respect to the law of the densities of the successive strata of the earth. Now the increase of gravity at the pole may be ascertained by means of observations with the pendulum in different latitudes. Hence it is plain that Clairaut's theorem furnishes a practical method for determining the value of the earth's ellipticity. — *Translator.*

around the sun; Newton established that they move under the immediate influence of the attractive force of that body, that they do not describe right lines, that, in fact, they obey the laws of Kepler. It was necessary, then, to prove that the orbits of comets are curves which return into themselves, or that the same comet has been seen on several distinct occasions. This discovery was reserved for Halley. By a minute investigation of the circumstances connected with the apparitions of all the comets to be met with in the records of history, in ancient chronicles, and in astronomical annals, this eminent philosopher was enabled to prove that the comets of 1682, of 1607, and of 1531 were in reality so many successive apparitions of one and the same body.

This identity involved a conclusion before which more than one astronomer shrunk. It was necessary to admit that the time of a complete revolution of the comet was subject to a great variation, amounting to as much as two years in seventy-six.

Were such great discordances due to the disturbing action of the planets?

The answer to this question would introduce comets into the category of ordinary planets or would exclude them for ever. The calculation was difficult: Clairaut discovered the means of effecting it. While success was still uncertain the illustrious geometer gave proof of the greatest boldness, for in the course of the year 1758 he undertook to determine the time of the following year when the comet of 1682 would reappear. He designated the constellations, nay the stars, which it would encounter in its progress.

This was not one of those remote predictions which astrologers and others formerly combined very skilfully with the tables of mortality, so that they might not be falsified during their lifetime: the event was close at hand. The question at issue was nothing less than the creation of a new era in cometary astronomy or the casting of a reproach upon science, the consequences of which it would long continue to feel.

Clairaut found by a long process of calculation, conducted with great skill, that the action of Jupiter and Saturn ought to have retarded the movement of the comet; that the time of revolution compared with that immediately preceding, would be increased 518 days by the disturbing action of Jupiter, and 100 days by the action of Saturn, forming a total of 618 days or more than a year and eight months.

Never did a question of astronomy excite a more intense, a more legitimate curiosity. All classes of society awaited with equal interest the announced apparition. A Saxon peasant, Palitzch, first perceived the comet. Henceforward, from one extremity of Europe to the other, a thousand telescopes traced

route was always, within the limits of precision of the calculations, that which Clairaut had indicated beforehand. The prediction of the illustrious geometer was verified in regard both to time and space: astronomy had just achieved a great and important triumph, and, as usual, had destroyed at one blow a disgraceful and inveterate prejudice. As soon as it was established that the returns of comets might be calculated beforehand, those bodies lost for ever their ancient prestige. The most timid minds troubled themselves quite as little about them as about eclipses of the sun and moon, which are equally subject to calculation. In fine, the labours of Clairaut had produced a deeper impression on the public mind than the learned, ingenious, and acute reasoning of Bayle.

The heavens offer to reflecting minds nothing more curious or more strange than the equality which subsists between the movements of rotation and revolution of our satellite. By reason of this perfect equality the moon always presents the same side to the earth. The hemisphere which we see in the present day is precisely that which our ancestors saw in the most remote ages; it is exactly the hemisphere which future generations will perceive.

The doctrine of final causes which certain philosophers have so abundantly made use of in endeavouring to account for a great number of natural phenomena was in this particular case totally inapplicable. In fact, how could it be pretended that mankind could have any interest in perceiving incessantly the same hemisphere of the moon, in never obtaining a glimpse of the opposite hemisphere? On the other hand, the existence of a perfect, mathematical equality between elements having no necessary connexion —such as the movements of translation and rotation of a given celestial body—was not less repugnant to all ideas of probability. There were besides two other numerical coincidences quite as extraordinary: an identity of direction, relative to the stars, of the equator and orbit of the moon; exactly the same precessional movements of these two planes. This group of singular phenomena, discovered by J. D. Cassini, constituted the mathematical code of what is called the *Libration of the Moon.*

The libration of the moon formed a very imperfect part of physical astronomy when Lagrange made it depend on a circumstance connected with the figure of our satellite which was not observable from the earth, and thereby connected it completely with the principles of universal gravitation.

At the time when the moon was converted into a solid body, the action of the earth compelled it to assume a less regular figure than if no attracting body had been situate in its vicinity. The action of our globe rendered elliptical an equator which otherwise would have been circular. This disturbing action did not prevent

the lunar equator from bulging out in every direction, but the prominence of the equatorial diameter directed towards the earth became four times greater than that of the diameter which we see perpendicularly.

The moon would appear then, to an observer situate in space and examining it transversely, to be elongated towards the earth, to be a sort of pendulum without a point of suspension. When a pendulum deviates from the vertical, the action of gravity brings it back; when the principal axis of the moon recedes from its usual direction, the earth in like manner compels it to return.

We have here, then, a complete explanation of a singular phenomenon, without the necessity of having recourse to the existence of an almost miraculous equality between two movements of translation and rotation, entirely independent of each other. Mankind will never see but one face of the moon. Observation had informed us of this fact; now we know further that this is due to a physical cause which may be calculated, and which is visible only to the mind's eye,—that it is attributable to the elongation which the diameter of the moon experienced when it passed from the liquid to the solid state under the attractive influence of the earth.

If there had existed originally a slight difference between the movements of rotation and revolution of the moon, the attraction of the earth would have reduced these movements to a rigorous equality. This attraction would have even sufficed to cause the disappearance of a slight want of coincidence in the intersections of the equator and orbit of the moon with the plane of the ecliptic.

The memoir in which Lagrange has so successfully connected the laws of libration with the principles of gravitation, is no less remarkable for intrinsic excellence than style of execution. After having perused this production, the reader will have no difficulty in admitting that the word *elegance* may be appropriately applied to mathematical researches.

In this analysis we have merely glanced at the astronomical discoveries of Clairaut, D'Alembert, and Lagrange. We shall be somewhat less concise in noticing the labours of Laplace.

After having enumerated the various forces which must result from the mutual action of the planets and satellites of our system, even the great Newton did not venture to investigate the general nature of the effects produced by them. In the midst of the labyrinth formed by increases and diminutions of velocity, variations in the forms of the orbits, changes of distances and inclinations, which these forces must evidently produce, the most learned geometer would fail to discover a trustworthy guide. This extreme

complication gave birth to a discouraging reflection. Forces so numerous, so variable in position, so different in intensity, seemed to be incapable of maintaining a condition of equilibrium except by a sort of miracle. Newton even went so far as to suppose that the planetary system did not contain within itself the elements of indefinite stability; he was of opinion that a powerful hand must intervene from time to time, to repair the derangements occasioned by the mutual action of the various bodies. Euler, although farther advanced than Newton in a knowledge of the planetary perturbations, refused also to admit that the solar system was constituted so as to endure for ever.

Never did a greater philosophical question offer itself to the inquiries of mankind. Laplace attacked it with boldness, perseverance, and success. The profound and long-continued researches of the illustrious geometer established with complete evidence that the planetary ellipses are perpetually variable; that the extremities of their major axes make the tour of the heavens; that, independently of an oscillatory motion, the planes of their orbits experience a displacement in virtue of which their intersections with the plane of the terrestrial orbit are each year directed towards different stars. In the midst of this apparent chaos there is one element which remains constant or is merely subject to small periodic changes: namely, the major axis of each orbit, and consequently the time of revolution of each planet. This is the element which ought to have chiefly varied, according to the learned speculations of Newton and Euler.

The principle of universal gravitation suffices for preserving the stability of the solar system. It maintains the forms and inclinations of the orbits in a mean condition which is subject to slight oscillations; variety does not entail disorder; the universe offers the example of harmonious relations, of a state of perfection which Newton himself doubted. This depends on circumstances which calculation disclosed to Laplace, and which, upon a superficial view of the subject, would not seem to be capable of exercising so great an influence. Instead of planets revolving all in the same direction in slightly eccentric orbits, and in planes inclined at small angles towards each other, substitute different conditions and the stability of the universe will again be put in jeopardy, and according to all probability there will result a frightful chaos.*

* The researches on the secular variations of the eccentricities and inclinations of the planetary orbits depend upon the solution of an algebraic equation equal in degree to the number of planets whose mutual action is considered, and the coefficients of which involve the values of the masses of those bodies. It may be shown that if the roots of this equation be equal or imaginary, the corresponding element, whether the eccentricity or the inclination, will increase

Although the invariability of the mean distances of the planetary orbits has been more completely demonstrated since the appearance of the memoir above referred to, that is to say by pushing the analytical approximations to a greater extent, it will, notwithstanding, always constitute one of the admirable discoveries of the author of the *Mécanique Céleste.* Dates, in the case of such subjects, are no luxury of erudition. The memoir in which Laplace communicated his results on the invariability of the mean motions or mean distances, is dated 1773.* It was in 1784 only, that he

indefinitely with the time in the case of each planet; but that if the roots, on the other hand, be real and unequal, the value of the element will oscillate in every instance within fixed limits. Laplace proved by a general analysis, that the roots of the equation are real and unequal, whence it followed that neither the eccentricity nor the inclination will vary in any case to an indefinite extent. But it still remained uncertain, whether the limits of oscillation were not in any instance so far apart that the variation of the element (whether the eccentricity or the inclination) might lead to a complete destruction of the existing physical condition of the planet. Laplace, indeed, attempted to prove, by means of two well-known theorems relative to the eccentricities and inclinations of the planetary orbits, that if those elements were once small, they would always remain so, provided the planets all revolved around the sun in one common direction and their masses were inconsiderable. It is to these theorems that M. Arago manifestly alludes in the text. Le Verrier and others have however remarked that they are inadequate to assure the permanence of the existing physical condition of several of the planets. In order to arrive at a definitive conclusion on this subject, it is indispensable to have recourse to the actual solution of the algebraic equation above referred to. This was the course adopted by the illustrious Lagrange in his researches on the secular variations of the planetary orbits. (*Mem. Acad. Berlin*, 1783–4.) Having investigated the values of the masses of the planets, he then determined, by an approximate solution, the values of the several roots of the algebraic equation upon which the variations of the eccentricities and inclinations of the orbits depended. In this way, he found the limiting values of the eccentricity and inclination for the orbit of each of the principal planets of the system. The results obtained by that great geometer have been mainly confirmed by the recent researches of Le Verrier on the same subject. (*Connaissance des Temps*, 1843.) — *Translator.*

* Laplace was originally led to consider the subject of the perturbations of the mean motions of the planets by his researches on the theory of Jupiter and Saturn. Having computed the numerical value of the secular inequality affecting the mean motion of each of those planets, neglecting the terms of the fourth and higher orders relative to the eccentricities and inclinations, he found it to be so small that it might be regarded as totally insensible. Justly suspecting that this circumstance was not attributable to the particular values of the elements of Jupiter and Saturn, he investigated the expression for the secular perturbation of the mean motion by a general analysis, neglecting, as before, the fourth and higher powers of the eccentricities and inclinations, and he found in this case, that the terms which were retained in the investigation absolutely destroyed each other, so that the expression was reduced to zero. In a memoir which he communicated to the Berlin Academy of Sciences, in 1776, Lagrange first showed that the mean distance (and consequently the mean

established the stability of the other elements of the system from the smallness of the planetary masses, the inconsiderable eccentricity of the orbits, and the revolution of the planets in one common direction around the sun.

The discovery of which I have just given an account to the reader excluded at least from the solar system the idea of the Newtonian attraction being a cause of disorder. But might not other forces, by combining with attraction, produce gradually increasing perturbations as Newton and Euler dreaded? Facts of a positive nature seemed to justify these fears.

A comparison of ancient with modern observations revealed the existence of a continual acceleration of the mean motions of the moon and the planet Jupiter, and an equally striking diminution of the mean motion of Saturn. These variations led to conclusions of the most singular nature.

In accordance with the presumed cause of these perturbations, to say that the velocity of a body increased from century to century was equivalent to asserting that the body continually approached the centre of motion: on the other hand, when the velocity diminished, the body must be receding from the centre.

Thus, by a strange arrangement of nature, our planetary system seemed destined to lose Saturn, its most mysterious ornament,— to see the planet accompanied by its ring and seven satellites, plunge gradually into unknown regions, whither the eye armed with the most powerful telescopes has never penetrated. Jupiter, on the other hand, the planet compared with which the earth is so insignificant, appeared to be moving in the opposite direction, so as to be ultimately absorbed in the incandescent matter of the sun. Finally the moon seemed as if it would one day precipitate itself upon the earth.

There was nothing doubtful or speculative in these sinister forebodings. The precise dates of the approaching catastrophes were alone uncertain. It was known, however, that they were very distant. Accordingly neither the learned dissertations of men of science nor the animated descriptions of certain poets produced any impression upon the public mind.

It was not so with our scientific societies the members of which regarded with regret the approaching destruction of our planetary system. The Academy of Sciences called the attention of geometers of all countries to these menacing perturbations. Euler and Lagrange descended into the arena. Never did their ma-

motion), was not affected by any secular inequalities, no matter what were the eccentricities or inclinations of the disturbing and disturbed planets. — *Translator.*

thematical genius shine with a brighter lustre. Still the question remained undecided. The inutility of such efforts seemed to suggest only a feeling of resignation on the subject, when from two disdained corners of the theories of analysis, the author of the *Mécanique Céleste* caused the laws of these great phenomena clearly to emerge. The variations of velocity of Jupiter, Saturn, and the Moon flowed then from evident physical causes, and entered into the category of ordinary periodic perturbations depending upon the principle of attraction. The variations in the dimensions of the orbits which were so much dreaded resolved themselves into simple oscillations included within narrow limits. Finally, by the powerful instrumentality of mathematical analysis, the physical universe was again established on a firm foundation.

I cannot quit this subject without at least alluding to the circumstances in the solar system upon which depend the so long unexplained variations of velocity of the Moon, Jupiter, and Saturn.

The motion of the earth around the sun is mainly effected in an ellipse the form of which is liable to vary from the effects of planetary perturbation. These alterations of form are periodic; sometimes the curve, without ceasing to be elliptic, approaches the form of a circle, while at other times it deviates more and more from that form. From the epoch of the earliest recorded observations, the eccentricity of the terrestrial orbit has been diminishing from year to year; at some future epoch the orbit, on the contrary, will begin to deviate from the form of a circle, and the eccentricity will increase to the same extent as it previously diminished, and according to the same laws.

Now Laplace has shown that the mean motion of the moon around the earth is connected with the form of the ellipse which the earth describes around the sun; that a diminution of the eccentricity of the ellipse inevitably induces an increase in the velocity of our satellite, and *vice versâ;* finally, that this cause suffices to explain the numerical value of the acceleration which the mean motion of the moon has experienced from the earliest ages down to the present time.*

* Mr. Adams has recently detected a remarkable oversight committed by Laplace and his successors in the analytical investigation of the expression for this inequality. The effect of the rectification rendered necessary by the researches of Mr. Adams will be to diminish by about one sixth the coefficient of the principal term of the secular inequality. This coefficient has for its multiplier the square of the number of centuries which have elapsed from a given epoch; its value was found by Laplace to be 10″·18. Mr. Adams has ascertained that it must be diminished by 1″·66. This result has recently been verified by the researches of M. Plana. Its effect will be to alter in some degree the calculations of ancient eclipses. The Astronomer Royal has stated in his last

The origin of the inequalities in the mean motions of Jupiter and Saturn will be, I hope, as easy to conceive.

Mathematical analysis has not served to represent in finite terms the values of the derangements which each planet experiences in its movement from the action of all the other planets. In the present state of science, this value is exhibited in the form of an indefinite series of terms diminishing rapidly in magnitude. In calculation it is usual to neglect such of those terms as correspond in the order of magnitude to quantities beneath the errors of observation. But there are cases in which the order of the term in the series does not decide whether it be small or great. Certain numerical relations between the primitive elements of the disturbing and disturbed planets may impart sensible values to terms which usually admit of being neglected. This case occurs in the perturbations of Saturn produced by Jupiter, and in those of Jupiter produced by Saturn. There exists between the mean motions of these two great planets a simple relation of commensurability, five times the mean motion of Saturn being, in fact, very nearly equal to twice the mean motion of Jupiter. It happens, in consequence, that certain terms, which would otherwise be very small, acquire from this circumstance considerable values. Hence arise in the movements of these two planets, inequalities of long duration which require more than 900 years for their complete development, and which represent with marvellous accuracy all the irregularities disclosed by observation.

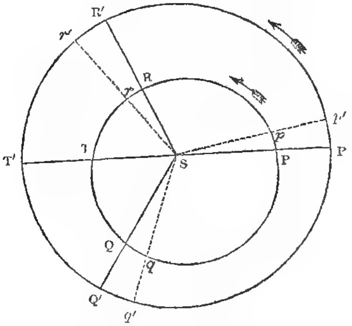
Is it not astonishing to find in the commensurability of the mean motions of two planets, a cause of perturbation of so influential a nature; to discover that the definitive solution of an immense difficulty — which baffled the genius of Euler, and which even led persons to doubt whether the theory of gravitation was capable of accounting for all the phenomena of the heavens — should depend upon the fortuitous circumstance of five times the mean motion of Saturn being equal to twice the mean motion of Jupiter? The beauty of the conception and the ultimate result are here equally worthy of admiration.*

Annual Report, to the Board of Visitors of the Royal Observatory, (June 7. 1856,) that steps have recently been taken at the Observatory, for calculating the various circumstances of those phenomena, upon the basis of the more correct data furnished by the researches of Mr. Adams. — *Translator.*

* The origin of this famous inequality may be best understood by reference to the mode in which the disturbing forces operate. Let P Q R, P′ Q′ R′ represent the orbits of Jupiter and Saturn, and let us suppose, for the sake of illustration, that they are both situate in the same plane. Let the planets be in conjunction at P, P′, and let them both be revolving around the sun S, in the direction represented by the arrows. Assuming that the mean motion of Jupiter is to that of Saturn exactly in the proportion of five to two, it follows that when Jupiter

We have just explained how Laplace demonstrated that the solar system can experience only small periodic oscillations around

has completed one revolution, Saturn will have advanced through two-fifths of a revolution. Similarly, when Jupiter has completed a revolution and a



half, Saturn will have effected three-fifths of a revolution. Hence when Jupiter arrives at T, Saturn will be a little in advance of T′. Let us suppose that the two planets come again into conjunction at Q, Q′. It is plain that while Jupiter has completed one revolution, and, advanced through the angle P S Q, (measured in the direction of the arrow), Saturn has simply described around S the angle P′S′Q′. Hence the *excess* of the angle described around S, by Jupiter, over the angle similarly described by Saturn, will amount to one complete revolution, or, 360°. But since the mean motions of the two planets are in the proportion of five to two, the angles described by them around S in any given time will be in the same proportion, and therefore the *excess* of the angle described by Jupiter over that described by Saturn will be to the angle described by Saturn in the proportion of three to two. But we have just found that the excess of these two angles in the present case amounts to 360°, and the angle described by Saturn is represented by P′ S′ Q′; consequently 360° is to the angle P′ S′ Q′ in the proportion of three to two, in other words P′ S′ Q′ is equal to two thirds of the circumference or 240°. In the same way it may be shown that the two planets will come into conjunction again at R, when Saturn has described another arc of 240°. Finally, when Saturn has advanced through a third arc of 240°, the two planets will come into conjunction at P, P′, the points whence they originally set out; and the two succeeding conjunctions will also manifestly occur at Q, Q′ and R, R′. Thus we see, that the conjunctions will always occur in three given points of the orbit of each planet situate at angular distances of 120° from each other. It is also obvious, that during the interval which elapses between the occurrence of two conjunctions in the same points of the orbits, and which includes three synodic revolutions of the planets, Jupiter will have accomplished five revolutions around the sun, and Saturn will have accomplished two revolutions. Now if the orbits of both planets were perfectly circular, the retarding and accelerating effects of the disturbing force of either planet would neutralise each other in the course of a synodic revolution, and therefore both planets would return to the same condition at each successive conjunction. But in consequence of the ellipticity of the orbits, the retarding effect of the disturbing force is manifestly no longer exactly compensated by the accelerative effect, and hence at the close of each synodic revolution, there remains a minute outstanding alteration in the movement of

a certain mean state. Let us now see in what way he succeeded in determining the absolute dimensions of the orbits.

What is the distance of the sun from the earth? No scientific question has occupied in a greater degree the attention of mankind; mathematically speaking, nothing is more simple. It suffices, as in common operations of surveying, to draw visual lines from the two extremities of a known base to an inaccessible object. The

each planet. A similar effect will be produced at each of the three points of conjunction; and as the perturbations which thus ensue do not generally compensate each other, there will remain a minute outstanding perturbation as the result of every three conjunctions. The effect produced being of the same kind (whether tending to accelerate or retard the movement of the planet) for every such triple conjunction, it is plain that the action of the disturbing forces would ultimately lead to a serious derangement of the movements of both planets. All this is founded on the supposition that the mean motions of the two planets are to each other as two to five; but in reality, this relation does not exactly hold. In fact while Jupiter requires 21,663 days to accomplish five revolutions, Saturn effects two revolutions in 21,518 days. Hence when Jupiter after completing his fifth revolution arrives at P, Saturn will have advanced a little beyond P′, and the conjunction of the two planets will occur at P, P′ when they have both described around S an additional arc of about 8°. In the same way it may be shown that the two succeeding conjunctions will take place at the points q, q', r, r' respectively 8° in advance of Q, Q′, R, R′. Thus we see that the points of conjunction will travel with extreme slowness in the same direction as that in which the planets revolve. Now since the angular distance between P and R is 120°, and since in a period of three synodic rovolutions or 21,758 days, the line of conjunction travels through an arc of 8°, it follows that in 892 years the conjunction of the two planets will have advanced from P, P′ to R, R′. In reality the time of travelling from P, P′ to R, R′ is somewhat longer from the indirect effects of planetary perturbation, amounting to 920 years. In an equal period of time the conjunction of the two planets will advance from Q, Q′ to R, R′ and from R, R′ to P, P′. During the half of this period the perturbative effect resulting from every triple conjunction will lie constantly in one direction, and during the other half it will lie in the contrary direction; that is to say, during a period of 460 years the mean motion of the disturbed planet will be continually accelerated, and in like manner, during an equal period it will be continually retarded. In the case of Jupiter disturbed by Saturn, the inequality in longitude amounts at its maximum to 21′; in the converse case of Saturn disturbed by Jupiter, the inequality is more considerable in consequence of the greater mass of the disturbing planet, amounting at its maximum to 49′. In accordance with the mechanical principle of the equality of action and reaction, it happens that while the mean motion of one planet is increasing, that of the other is diminishing, and *vice versâ*. We have supposed that the orbits of both planets are situate in the same plane. In reality, however, they are inclined to each other, and this circumstance will produce an effect exactly analogous to that depending on the eccentricities of the orbits. It is plain that the more nearly the mean motions of the two planets approach a relation of commensurability, the smaller will be the displacement of every third conjunction, and consequently the longer will be the duration, and the greater the ultimate accumulation, of the inequality. — *Translator.*

remainder is a process of elementary calculation. Unfortunately, in the case of the sun the distance is great and the bases which can be measured upon the earth are comparatively very small. In such a case the slightest errors in the direction of the visual lines exercise an enormous influence upon the results.

In the beginning of the last century Halley remarked that certain interpositions of Venus between the earth and the sun, or to use an expression applied to such conjunctions, that the *transits* of the planet across the sun's disk, would furnish at each observatory an indirect means of fixing the position of the visual ray very superior in accuracy to the most perfect direct methods.*

Such was the object of the scientific expeditions undertaken in 1761 and 1769, on which occasions France, not to speak of stations in Europe, was represented at the Isle of Rodrigo by Pingré, at the Isle of St. Domingo by Fleurin, at California by the Abbé Chappe, at Pondicherry by Legentil. At the same epochs England sent Maskelyne to St. Helena, Wales to Hudson's Bay, Mason to the Cape of Good Hope, Captain Cooke to Otaheite, &c. The observations of the southern hemisphere compared with those of Europe, and especially with the observations made by an Austrian astronomer Father Hell at Wardhus in Lapland, gave for the distance of the sun the result which has since figured in all treatises on astronomy and navigation.

No government hesitated in furnishing Academies with the means, however expensive they might be, of conveniently establishing their observers in the most distant regions. We have already remarked that the determination of the contemplated distance appeared to demand imperiously an extensive base, for small bases would have been totally inadequate to the purpose. Well, Laplace has solved the problem numerically without a base of any kind whatever; he has deduced the distance of the sun from observations of the moon made in one and the same place!

The sun is, with respect to our satellite, the cause of perturbations which evidently depend on the distance of the immense luminous globe from the earth. Who does not see that these perturbations would diminish if the distance increased; that they would increase on the contrary, if the distance diminished; that the distance finally determines the magnitude of the perturbations?

Observation assigns the numerical value of these perturbations; theory, on the other hand, unfolds the general mathematical relation which connects them with the solar parallax, and with other

* The utility of observations of the transits of the inferior planets for determining the solar parallax, was first pointed out by James Gregory (*Optica Promota*, 1663).—*Translator.*

known elements. The determination of the mean radius of the terrestrial orbit then becomes one of the most simple operations of algebra. Such is the happy combination by the aid of which Laplace has solved the great, the celebrated problem of parallax. It is thus that the illustrious geometer found for the mean distance of the sun from the earth, expressed in radii of the terrestrial orbit, a value differing only in a slight degree from that which was the fruit of so many troublesome and expensive voyages. According to the opinion of very competent judges the result of the indirect method might not impossibly merit the preference.*

The movements of the moon proved a fertile mine of research to our great geometer. His penetrating intellect discovered in them unknown treasures. He disentangled them from everything which concealed them from vulgar eyes with an ability and a perseverance equally worthy of admiration. The reader will excuse me for citing another of such examples.

The earth governs the movements of the moon. The earth is flattened, in other words its figure is spheroidal. A spheroidal body does not attract like a sphere. There ought then to exist in the movement, I had almost said in the countenance of the moon, a sort of impression of the spheroidal figure of the earth. Such was the idea as it originally occurred to Laplace.

It still remained to ascertain (and here consisted the chief difficulty), whether the effects attributable to the spheroidal figure of the earth were sufficiently sensible not to be confounded with the errors of observation. It was accordingly necessary to find the general formula of perturbations of this nature, in order to be able, as in the case of the solar parallax, to eliminate the unknown quantity.

The ardour of Laplace, combined with his power of analytical research, surmounted all obstacles. By means of an investigation which demanded the most minute attention, the great geometer discovered in the theory of the moon's movements, two well-defined perturbations depending on the spheroidal figure of the earth. The first affected the resolved element of the motion of our satellite which is chiefly measured with the instrument known in observatories by the name of the transit instrument; the second, which operated in the direction north and south, could only be effected by

* Mayer, from the principles of gravitation (*Theoria Lunæ*, 1767), computed the value of the solar parallax to be 7″·8. He remarked that the error of this determination did not amount to one twentieth of the whole, whence it followed that the true value of the parallax could not exceed 8″·2. Laplace, by an analogous process, determined the parallax to be 8″·45. Encke, by a profound discussion of the observations of the transits of Venus in 1761 and 1769, found the value of the same element to be 8″·5776. — *Translator.*

observations with a second instrument termed the mural circle. These two inequalities of very different magnitudes connected with the cause which produces them by analytical combinations of totally different kinds have, however, both conducted to the same value of the ellipticity. It must be borne in mind, however, that the ellipticity thus deduced from the movements of the moon, is not the ellipticity corresponding to such or such a country, the ellipticity observed in France, in England, in Italy, in Lapland, in North America, in India, or in the region of the Cape of Good Hope, for the earth's materials having undergone considerable upheavings at different times and in different places, the primitive regularity of its curvature has been sensibly disturbed by this cause. The moon, and it is this circumstance which renders the result of such inestimable value, ought to assign, and has in reality assigned, the general ellipticity of the earth; in other words, it has indicated a sort of mean value of the various determinations obtained at enormous expense, and with infinite labour, as the result of long voyages undertaken by astronomers of all the countries of Europe.

I shall add a few brief remarks, for which I am mainly indebted to the author of the *Mécanique Céleste*. They seem to be eminently adapted for illustrating the profound, the unexpected, and almost paradoxical character of the methods which I have just attempted to sketch.

What are the elements which it has been found necessary to confront with each other in order to arrive at results expressed even to the precision of the smallest decimals?

On the one hand, mathematical formulæ deduced from the principle of universal attraction; on the other hand, certain irregularities observed in the returns of the moon to the meridian.

An observing geometer who, from his infancy, had never quitted his chamber of study, and who had never viewed the heavens except through a narrow aperture directed north and south, in the vertical plane of which the principal astronomical instruments are made to move, — to whom nothing had ever been revealed respecting the bodies revolving above his head, except that they attract each other according to the Newtonian law of gravitation,— would, however, be enabled to ascertain that his narrow abode was situated upon the surface of a spheroidal body, the equatorial axis of which surpassed the polar axis by a *three hundred and sixth part;* he would have also found, in his isolated immoveable position, his true distance from the sun.

I have stated at the commencement of this Notice, that it is to D'Alembert we owe the first satisfactory mathematical explanation of the phenomenon of the precession of the equinoxes. But our

llustrious countryman, as well as Euler whose solution appeared ubsequently to that of D'Alembert, omitted all consideration of ;ertain physical circumstances which, however, did not seem to be of a nature to be neglected without examination. Laplace has ·upplied this deficiency. He has shown that the sea, notwithstanding its fluidity, and that the atmosphere, notwithstanding its currents, exercise the same influence on the movements of the terrestrial axis as if they formed solid masses adhering to the terrestrial spheroid.

Do the extremities of the axis around which the earth performs an entire revolution once in every twenty-four hours, correspond always to the same material points of the terrestrial spheroid? In other words, do the poles of rotation, which from year to year correspond to different stars, undergo also a displacement at the surface of the earth?

In the case of the affirmative, the equator is moveable as well as the poles; the terrestrial latitudes are variable; no country during the lapse of ages will enjoy, even on an average, a constant climate; regions the most different will, in their turn, become circumpolar. Adopt the contrary supposition, and everything assumes the character of an admirable permanence.

The question which I have just suggested, one of the most important in Astronomy, cannot be solved by the aid of mere observation on account of the uncertainty of the early determinations of terrestrial latitude. Laplace has supplied this defect by analysis. The great geometer has demonstrated that no circumstance depending on universal gravitation can sensibly displace the poles of the earth's axis relatively to the surface of the terrestrial spheroid. The sea, far from being an obstacle to the invariable rotation of the earth upon its axis, would, on the contrary, reduce the axis to a permanent condition in consequence of the mobility of the waters and the resistance which their oscillations experience.

The remarks which I have just made with respect to the position of the terrestrial axis are equally applicable to the time of the earth's rotation which is the unit, the true standard of time. The importance of this element induced Laplace to examine whether its numerical value might not be liable to vary from internal causes such as earthquakes and volcanoes. It is hardly necessary for me to state that the result obtained was negative.

The admirable memoir of Lagrange upon the libration of the moon seemed to have exhausted the subject. This, however, was not the case.

The motion of revolution of our satellite around the earth is subject to perturbations, technically termed *secular*, which were either unknown to Lagrange or which he neglected. These ine-

qualities eventually place the body, not to speak of entire circumferences, at angular distances of a semi-circle, a circle and a half, &c. from the position which it would otherwise occupy. If the movement of rotation did not participate in such perturbations, the moon in the lapse of ages would present in succession all the parts of its surface to the earth.

This event will not occur. The hemisphere of the moon which is actually invisible, will remain invisible for ever. Laplace, in fact, has shown that the attraction of the earth introduces into the rotatory motion of the lunar spheroid the secular inequalities which exist in the movement of revolution.

Researches of this nature exhibit in full relief the power of mathematical analysis. It would have been very difficult to have discovered by synthesis truths so profoundly enveloped in the complex action of a multitude of forces.

We should be inexcusable if we omitted to notice the high importance of the labours of Laplace on the improvement of the lunar tables. The immediate object of this improvement was, in effect, the promotion of maritime intercourse between distant countries, and, what was indeed far superior to all considerations of mercantile interest, the preservation of the lives of mariners.

Thanks to a sagacity without parallel, to a perseverance which knew no limits, to an ardour always youthful and which communicated itself to able coadjutors, Laplace solved the celebrated problem of the longitude more completely than could have been hoped for in a scientific point of view, with greater precision than the art of navigation in its utmost refinement demanded. The ship, the sport of the winds and tempests, has no occasion, in the present day, to be afraid of losing itself in the immensity of the ocean. An intelligent glance at the starry vault indicates to the pilot, in every place and at every time, his distance from the meridian of Paris. The extreme perfection of the existing tables of the moon entitles Laplace to be ranked among the benefactors of humanity.*

* The theoretical researches of Laplace formed the basis of Burckhardt's Lunar Tables, which are chiefly employed in computing the places of the moon for the Nautical Almanac and other Ephemerides. These tables were defaced by an empiric equation, suggested for the purpose of representing an inequality of long period which seemed to affect the mean longitude of the moon. No satisfactory explanation of the origin of this inequality could be discovered by any geometer, although it formed the subject of much toilsome investigation throughout the present century, until at length M. Hansen found it to arise from a combination of two inequalities due to the disturbing action of Venus. The period of one of these inequalities is 273 years, and that of the other is 239 years. The maximum value of the former is $27''{\cdot}4$, and that of the latter is $23''{\cdot}2$. — *Translator.*

In the beginning of the year 1611, Galileo supposed that he found in the eclipses of Jupiter's satellites a simple and rigorous solution of the famous problem of the longitude, and active negotiations were immediately commenced with the view of introducing the new method on board the numerous vessels of Spain and Holland. These negotiations failed. From the discussion it plainly appeared that the accurate observation of the eclipses of the satellites would require powerful telescopes; but such telescopes could not be employed on board a ship tossed about by the waves.

The method of Galileo seemed, at any rate, to retain all its advantages when applied on land, and to promise immense improvements to geography. These expectations were found to be premature. The movements of the satellites of Jupiter are not by any means so simple as the immortal inventor of the method of longitudes supposed them to be. It was necessary that three generations of astronomers and mathematicians should labour with perseverance in unfolding their most considerable perturbations. It was necessary, in fine, that the tables of those bodies should acquire all desirable and necessary precision, that Laplace should introduce into the midst of them the torch of mathematical analysis.

In the present day, the nautical ephemerides contain, several years in advance, the indication of the times of the eclipses and reappearances of Jupiter's satellites. Calculation does not yield in precision to direct observation. In this group of satellites, considered as an independent system of bodies, Laplace found a series of perturbations analogous to those which the planets experience. The rapidity of the revolutions unfolds, in a sufficiently short space of time, changes in this system which require centuries for their complete development in the solar system.

Although the satellites exhibit hardly an appreciable diameter even when viewed in the best telescopes, our illustrious countryman was enabled to determine their masses. Finally, he discovered certain simple relations of an extremely remarkable character between the movements of those bodies, which have been called *the laws of Laplace.* Posterity will not obliterate this designation; it will acknowledge the propriety of inscribing in the heavens the name of so great an astronomer beside that of Kepler.

Let us cite two or three of the laws of Laplace: —

If we add to the mean longitude of the first satellite twice that of the third, and substract from the sum three times the mean longitude of the second, the result will be exactly equal to 180°.

Would it not be very extraordinary if the three satellites had been placed originally at the distances from Jupiter, and in the

positions, with respect to each other, adapted for constantly and rigorously maintaining the foregoing relation? Laplace has replied to this question by showing that it is not necessary that this relation should have been rigorously true at the origin. The mutual action of the satellites would necessarily have reduced it to its present mathematical condition, if once the distances and the positions satisfied the law approximately.

This first law is equally true when we employ the synodical elements. It hence plainly results, that the three first satellites of Jupiter can never be all eclipsed at the same time. Bearing this in mind, we shall have no difficulty in apprehending the import of a celebrated observation of recent times, during which certain astronomers perceived the planet for a short time without any of his four satellites. This would not by any means authorise us in supposing the satellites to be eclipsed. A satellite disappears when it is projected upon the central part of the luminous disk of Jupiter, and also when it passes behind the opaque body of the planet.

The following is another very simple law to which the mean motions of the same satellites of Jupiter are subject:

If we add to the mean motion of the first satellite twice the mean motion of the third, the sum is exactly equal to three times the mean motion of the second.*

This numerical coincidence, which is perfectly accurate, would be one of the most mysterious phenomena in the system of the

* This law is necessarily included in the law already enunciated by the author relative to the mean longitudes. The following is the most usual mode of expressing these curious relations: 1st, the mean motion of the first satellite, plus twice the mean motion of the third, minus three times the mean motion of the second, is rigorously equal to zero; 2nd, the mean longitude of the first satellite, plus twice the mean longitude of the third, minus three times the mean longitude of the second, is equal to 180°. It is plain that if we only consider the mean longitude here to refer to a *given epoch*, the combination of the two laws will assure the existence of an analogous relation between the mean longitudes *for any instant of time whatever*, whether past or future. Laplace has shown, as the author has stated in the text, that if these relations had only been approximately true at the origin, the mutual attraction of the three satellites would have ultimately rendered them rigorously so; under such circumstances, the mean longitude of the first satellite, plus twice the mean longitude of the third, minus three times the mean longitude of the second, would continually oscillate about 180° as a mean value. The three satellites would participate in this libratory movement, the extent of oscillation depending in each case on the mass of the satellite and its distance from the primary, but the period of libration is the same for all the satellites, amounting to 2,270 days 18 hours, or rather more than six years. Observations of the eclipses of the satellites have not afforded any indications of the actual existence of such a libratory motion, so that the relations between the mean motions and mean longitudes may be presumed to be always rigorously true. — *Translator.*

universe if Laplace had not proved that the law need only hav been approximate at the origin, and that the mutual action of th satellites has sufficed to render it rigorous.

The illustrious geometer, who always pursued his researches t their most remote ramifications, arrived at the following result The action of Jupiter regulates the movements of rotation of th satellites so that, without taking into account the secular pertur bations, the time of rotation of the first satellite plus twice th time of rotation of the third, forms a sum which is constantl equal to three times the time of rotation of the second.

Influenced by a deference, a modesty, a timidity, without an plausible motive, our artists in the last century surrendered to th English the exclusive privilege of constructing instruments c astronomy. Thus, let us frankly acknowledge the fact, at th time when Herschel was prosecuting his beautiful observations o the other side of the Channel, there existed in France no instru ments adapted for developing them; we had not even the mean of verifying them. Fortunately for the scientific honour of ou country, mathematical analysis is also a powerful instrumen Laplace gave ample proof of this on a memorable occasion whe from the retirement of his chamber he predicted, he minutel announced, what the excellent astronomer of Windsor would se with the largest telescopes which were ever constructed by th hand of man.

When Galileo, in the beginning of the year 1610, directe towards Saturn a telescope of very low power which he had jus executed with his own hands, he perceived that the planet wa not an ordinary globe, without however being able to ascertain it real form. The expression *tri-corporate* by which the illustriou Florentine designated the appearance of the planet, implied eve a totally erroneous idea of its structure. Our countryman Rober val entertained much sounder views on the subject, but from no having instituted a detailed comparison between his hypothes and the results of observation, he abandoned to Huyghens th honour of being regarded as the author of the true theory of th phenomena presented by the wonderful planet.

Every person knows in the present day, that Saturn consists of globe about 900 times greater than the earth and a ring. This rin does not touch the ball of the planet, being everywhere remove from it at a distance of 20,000 (English) miles. Observatio indicates the breadth of the ring to be 54,000 miles. The thick ness certainly does not exceed 250 miles. With the exception c a black streak which divides the ring throughout its whole contou into two parts of unequal breadth and of different brightnes this strange colossal bridge without piles had never offered t

the most experienced or skilful observers either spot or protuberance adapted for deciding whether it was immoveable or endued with a movement of rotation.

Laplace considered it to be very improbable, if the ring was immoveable, that its constituent parts should be capable of resisting by their mere cohesion the continual attraction of the planet. A movement of rotation occurred to his mind as constituting the principle of stability, and he hence deduced the necessary velocity. The velocity thus found was exactly equal to that which Herschel subsequently deduced from a course of extremely delicate observations.

The two parts of the ring being placed at different distances from the planet, could not fail to experience from the action of the sun, different movements of rotation. It would hence seem that the planes of both rings ought to be generally inclined towards each other, whereas they appear from observation always to coincide. It was necessary then that some physical cause should exist which would be capable of neutralising the action of the sun. In a memoir published in February 1789, Laplace found that this cause mnst reside in the ellipticity of Saturn produced by a rapid movement of rotation of the planet, a movement the existence of which Herschel announced in November 1789.

The reader cannot fail to remark how, on certain occasions, the eyes of the mind can supply the want of the most powerful telescopes, and lead to astronomical discoveries of the highest importance.

Let us descend from the heavens upon the earth. The discoveries of Laplace will appear not less important, not less worthy of his genius.

The phenomena of the tides, which an ancient philosopher designated in despair as *the tomb of human curiosity*, were connected by Laplace with an analytical theory in which the physical conditions of the question figure for the first time. Accordingly calculators, to the immense advantage of the navigation of our maritime coasts, venture in the present day to predict several years in advance the details of the time and height of the full tides without more anxiety respecting the result than if the question related to the phases of an eclipse.

There exists between the different phenomena of the ebb and flow of the tides and the attractive forces which the sun and moon exercise upon the fluid sheet which covers three fourths of the globe, an intimate and necessary connexion from which Laplace, by the aid of a series of twenty years of observations executed at Brest, deduced the value of the mass of our satellite. Science knows in the present day that seventy-five moons would be necessary to

form a weight equivalent to that of the terrestrial globe, and it is indebted for this result to an attentive and minute study of the oscillations of the ocean. We know only one means of enhancing the admiration which every thoughtful mind will entertain for theories capable of leading to such conclusions. An historical statement will supply it. In the year 1631, the illustrious Galileo, as appears from his *Dialogues*, was so far from perceiving the mathematical relations from which Laplace deduced results so beautiful, so unequivocal, and so useful, that he taxed with frivolousness the vague idea which Kepler entertained of attributing to the moon's attraction a certain share in the production of the diurnal and periodical movements of the waters of the ocean.

Laplace did not confine himself to extending so considerably, and improving so essentially, the mathematical theory of the tides; he considered the phenomenon from an entirely new point of view; it was he who first treated of the stability of the ocean. Systems of bodies, whether solid or fluid, are subject to two kinds of equilibrium, which we must carefully distinguish from each other. In the case of stable equilibrium the system, when slightly disturbed, tends always to return to its original condition. On the other hand, when the system is in unstable equilibrium, a very insignificant derangement might occasion an enormous dislocation in the relative positions of its constituent parts.

If the equilibrium of waves is of the latter kind, the waves engendered by the action of winds, by earthquakes, and by sudden movements from the bottom of the ocean, have perhaps risen in past times and may rise in the future to the height of the highest mountains. The geologist will have the satisfaction of deducing from these prodigious oscillations a rational explanation of a great multitude of phenomena, but the public will thereby be exposed to new and terrible catastrophes.

Mankind may rest assured: Laplace has proved that the equilibrium of the ocean is stable, but upon the express condition (which, however, has been amply verified by established facts), that the mean density of the fluid mass is less than the mean density of the earth. Everything else remaining the same, let us substitute an ocean of mercury for the actual ocean, and the stability will disappear, and the fluid will frequently surpass its boundaries, to ravage continents even to the height of the snowy regions which lose themselves in the clouds.

Does not the reader remark how each of the analytical investigations of Laplace serves to disclose the harmony and duration of the universe and of our globe!

It was impossible that the great geometer, who had succeeded so well in the study of the tides of the ocean, should not have

occupied his attention with the tides of the atmosphere; that he should not have submitted to the delicate and definitive tests of a rigorous calculus, the generally diffused opinions respecting the influence of the moon upon the height of the barometer and other meteorological phenomena.

Laplace, in effect, has devoted a chapter of his splendid work to an examination of the oscillations which the attractive force of the moon is capable of producing in our atmosphere. It results from these researches, that, at Paris, the lunar tide produces no sensible effect upon the barometer. The height of the tide, obtained by the discussion of a long series of observations, has not exceeded two-hundredths of a millimètre, a quantity which, in the present state of meteorological science, is less than the probable error of observation.

The calculation to which I have just alluded, may be cited in support of considerations to which I had recourse when I wished to establish, that if the moon alters more or less the height of the barometer, according to its different phases, the effect is not attributable to attraction.

No person was more sagacious than Laplace in discovering intimate relations between phenomena apparently very dissimilar; no person showed himself more skilful in deducing important conclusions from those unexpected affinities.

Towards the close of his days, for example, he overthrew with a stroke of the pen, by the aid of certain observations of the moon, the cosmogonic theories of Buffon and Bailly, which were so long in favour.

According to these theories, the earth was inevitably advancing to a state of congelation which was close at hand. Laplace, who never contented himself with a vague statement, sought to determine in numbers the rapid cooling of our globe which Buffon had so eloquently but so gratuitously announced. Nothing could be more simple, better connected, or more demonstrative, than the chain of deductions of the celebrated geometer.

A body diminishes in volume when it cools. According to the most elementary principles of mechanics, a rotating body which contracts in dimensions ought inevitably to turn upon its axis with greater and greater rapidity. The length of the day has been determined in all ages by the time of the earth's rotation; if the earth is cooling, the length of the day must be continually shortening. Now there exists a means of ascertaining whether the length of the day has undergone any variation: this consists in examining, for each century, the arc of the celestial sphere described by the moon during the interval of time which the astronomers of the existing epoch called a day,—in other words,

the time required by the earth to effect a complete rotation on its axis, the velocity of the moon being in fact independent of the time of the earth's rotation.

Let us now, after the example of Laplace, take from the standard tables the least considerable values, if you choose, of the expansions or contractions which solid bodies experience from changes of temperature; search then the annals of Grecian, Arabian, and modern astronomy for the purpose of finding in them the angular velocity of the moon, and the great geometer will prove, by incontrovertible evidence founded upon these data, that during a period of 2000 years the mean temperature of the earth has not varied to the extent of the hundredth part of a degree of the centigrade thermometer. No eloquent declamation is capable of resisting such a process of reasoning, or withstanding the force of such numbers. The mathematics have been in all ages the implacable adversaries of scientific romances.

The fall of bodies, if it was not a phenomenon of perpetual occurrence, would justly excite in the highest degree the astonishment of mankind. What, in effect, is more extraordinary than to see an inert mass, that is to say, a mass deprived of will, a mass which ought not to have any propensity to advance in one direction more than in another, precipitate itself towards the earth as soon as it ceased to be supported!

Nature engenders the gravity of bodies by a process so recondite, so completely beyond the reach of our senses and the ordinary resources of human intelligence, that the philosophers of antiquity, who supposed that they could explain everything mechanically according to the simple evolutions of atoms, excepted gravity from their speculations.

Descartes attempted what Leucippus, Democritus, Epicurus, and their followers thought to be impossible.

He made the fall of terrestrial bodies depend upon the action of a vortex of very subtle matter circulating around the earth. The real improvements which the illustrious Huyghens applied to the ingenious conception of our countryman were far, however, from imparting to it clearness and precision, those characteristic attributes of truth.

Those persons form a very imperfect estimate of the meaning of one of the greatest questions which has occupied the attention of modern inquirers, who regard Newton as having issued victorious from a struggle in which his two immortal predecessors had failed. Newton did not discover the cause of gravity any more than Galileo did. Two bodies placed in juxtaposition approach each other. Newton does not inquire into the nature of the force which produces

this effect. The force exists, he designates it by the term attraction; but, at the same time, he warns the reader that the term as thus used by him does not imply any definite idea of the physical process by which gravity is brought into existence and operates.

The force of attraction being once admitted as a fact, Newton studies it in all terrestrial phenomena, in the revolutions of the moon, the planets, satellites, and comets; and, as we have already stated, he deduced from this incomparable study the simple, universal, mathematical characteristics of the forces which preside over the movements of all the bodies of which our solar system is composed.

The applause of the scientific world did not prevent the immortal author of the *Principia* from hearing some persons refer the principle of gravitation to the class of occult qualities. This circumstance induced Newton and his most devoted followers to abandon the reserve which they had hitherto considered it their duty to maintain. Those persons were then charged with ignorance who regarded attraction as an essential property of matter, as the mysterious indication of a sort of charm; who supposed that two bodies may act upon each other without the intervention of a third body. This force was then either the result of the tendency of an ethereal fluid to move from the free regions of space, where its density is a maximum, towards the planetary bodies around which there exists a greater degree of rarefaction, or the consequence of the impulsive force of some fluid medium.

Newton never expressed a definitive opinion respecting the origin of the impulse which occasioned the attractive force of matter, at least in our solar system. But we have strong reasons for supposing, in the present day, that in using the word *impulse*, the great geometer was thinking of the systematic ideas of Varignon and Fatio de Duillier, subsequently re-invented and perfected by Lesage: these ideas, in effect, had been communicated to him before they were published to the world.

According to Lesage, there are, in the regions of space, bodies moving in every possible direction, and with excessive rapidity. The author applied to these the name of ultra-mundane corpuscules. Their totality constituted the gravitative fluid, if indeed, the designation of a fluid be applicable to an assemblage of particles having no mutual connexion.

A single body placed in the midst of such an ocean of moveable particles, would remain at rest although it were impelled equally in every direction. On the other hand, two bodies ought to advance towards each other, since they would serve the purpose of mutual screens, since the surfaces facing each other would no longer be hit in the direction of their line of junction by the ultra-

mundane particles, since there would then exist currents, the effect of which would no longer be neutralised by opposite currents. It will be easily seen, besides, that two bodies plunged into the gravitative fluid, would tend to approach each other with an intensity which would vary in the inverse proportion of the square of the distance.

If attraction is the result of the impulse of a fluid, its action ought to employ a finite time in traversing the immense spaces which separate the celestial bodies. If the sun, then, were suddenly extinguished, the earth after the catastrophe would, mathematically speaking, still continue for some time to experience its attractive influence. The contrary would happen on the occasion of the sudden birth of a planet: a certain time would elapse before the attractive force of the new body would make itself felt on the earth.

Several geometers of the last century were of opinion that the force of attraction is not transmitted instantaneously from one body to another; they even assigned to it a comparatively inconsiderable velocity of propagation. Daniel Bernoulli, for example, in attempting to explain how the spring tide arrives upon our coasts a day and a half after the sizygees, that is to say, a day and a half after the epochs when the sun and moon are most favourably situated for the production of this magnificent phenomenon, assumed that the disturbing force required all this time (a day and a half) for its propagation from the moon to the ocean. So feeble a velocity was inconsistent with the mechanical explanation of attraction of which we have just spoken. The explanation, in effect, necessarily supposes that the proper motions of the celestial bodies are insensible compared with the motion of the gravitative fluid.

After having discovered that the diminution of the eccentricity of the terrestrial orbit is the real cause of the observed acceleration of the motion of the moon, Laplace, on his part, endeavoured to ascertain whether this mysterious acceleration did not depend on the gradual propagation of attraction.

The result of calculation was at first favourable to the plausibility of the hypothesis. It showed that the gradual propagation of the attractive force would introduce into the movement of our satellite a perturbation proportional to the square of the time which elapsed from the commencement of any epoch; that in order to represent numerically the results of astronomical observations it would not be necessary to assign a feeble velocity to attraction; that a propagation eight millions of times more rapid than that of light would satisfy all the phenomena.

Although the true cause of the acceleration of the moon is now well known, the ingenious calculation of which I have just spoken does not the less on that account maintain its place in science. In a mathematical point of view, the perturbation depending on the gradual propagation of the attractive force which this calculation indicates has a certain existence. The connexion between the velocity of perturbation and the resulting inequality is such that one of the two quantities leads to a knowledge of the numerical value of the other. Now, upon assigning to the inequality the greatest value which is consistent with the observations after they have been corrected for the effect due to the variation of the eccentricity of the terrestrial orbit, we find the velocity of the attractive force to be fifty millions of times the velocity of light!

If it be borne in mind, that this number is an inferior limit, and that the velocity of the rays of light amounts to 77,000 leagues (192,000 English miles) per second, the philosophers who profess to explain the force of attraction by the impulsive energy of a fluid, will see what prodigious velocities they must satisfy.

The reader cannot fail again to remark the sagacity with which Laplace singled out the phenomena which were best adapted for throwing light upon the most obscure points of celestial physics; nor the success with which he explored their various parts, and deduced from them numerical conclusions in presence of which the mind remains confounded.

The author of the *Mécanique Céleste* supposed, like Newton, that light consists of material molecules of excessive tenuity and endued in empty space with a velocity of 77,000 leagues in a second. However, it is right to warn those who would be inclined to avail themselves of this imposing authority, that the principal argument of Laplace, in favour of the system of emission, consisted in the advantage which it afforded of submitting every question to a process of simple and rigorous calculation; whereas, on the other hand, the theory of undulations has always offered immense difficulties to analysts. It was natural that a geometer who had so elegantly connected the laws of simple refraction which light undergoes in its passage through the atmosphere, and the laws of double refraction which it is subject to in the course of its passage through certain crystals, with the action of attractive and repulsive forces, should not have abandoned this route, before he recognised the impossibility of arriving by the same path, at plausible explanations of the phenomena of diffraction and polarisation. In other respects, the care which Laplace always employed in pursuing his researches, as far as possible, to their numerical results, will enable those who are disposed to institute a complete comparison between the two rival theories of light, to

derive from the *Mécanique Céleste* the materials of several interesting relations.

Is light an emanation from the sun? Does this body launch out incessantly in every direction a part of its own substance? Is it gradually diminishing in volume and mass? The attraction exercised by the sun upon the earth will, in that case, gradually become less and less considerable. The radius of the terrestrial orbit, on the other hand, cannot fail to increase, and a corresponding effect will be produced on the length of the year.

This is the conclusion which suggests itself to every person upon a first glance at the subject. By applying analysis to the question, and then proceeding to numerical computations, founded upon the most trustworthy results of observation relative to the length of the year in different ages, Laplace has proved that an incessant emission of light, going on for a period of two thousand years, has not diminished the mass of the sun by the two-millionth part of its original value.

Our illustrious countryman never proposed to himself anything vague or indefinite. His constant object was the explanation of the great phenomena of nature, according to the inflexible principles of mathematical analysis. No philosopher, no mathematician, could have maintained himself more cautiously on his guard against a propensity to hasty speculation. No person dreaded more the scientific errors which the imagination gives birth to, when it ceases to remain within the limits of facts, of calculation, and of analogy. Once, and once only, did Laplace launch forward, like Kepler, like Descartes, like Leibnitz, like Buffon, into the region of conjectures. His conception was not then less than a cosmogony.

All the planets revolve around the sun, from west to east, and in planes which include angles of inconsiderable magnitude.

The satellites revolve around their respective primaries in the same direction as that in which the planets revolve around the sun, that is to say, from west to east.

The planets and satellites which have been found to have a rotatory motion, turn also upon their axes from west to east. Finally, the rotation of the sun is also directed from west to east. We have here then an assemblage of forty-three movements, all operating in the same direction. By the calculus of probabilities, the odds are four thousand millions to one, that this coincidence in the direction of so many movements is not the effect of accident.

It was Buffon, I think, who first attempted to explain this singular feature of our solar system. Having wished, in the explanation of phenomena, to avoid all recourse to causes which were not warranted by nature, the celebrated academician investigated

a physical origin of the system in what was common to the movements of so many bodies differing in magnitude, in form, and in distance from the principal centre of attraction. He imagined that he discovered such an origin by making this triple supposition: a comet fell obliquely upon the sun; it pushed before it a torrent of fluid matter; this substance transported to a greater or less distance from the sun according to its mass formed by concentration all the known planets.

The bold hypothesis of Buffon is liable to unsurmountable difficulties. I proceed to indicate, in a few words, the cosmogonic system which Laplace substituted for that of the illustrious author of the *Histoire Naturelle.*

According to Laplace, the sun was at a remote epoch the central nucleus of an immense nebula, which possessed a very high temperature, and extended far beyond the region in which Uranus revolves in the present day. No planet was then in existence.

The solar nebula was endued with a general movement of revolution directed from west to east. As it cooled it could not fail to experience a gradual condensation, and, in consequence, to rotate with greater and greater rapidity. If the nebulous matter extended originally in the plane of the equator as far as the limit at which the centrifugal force exactly counterbalanced the attraction of the nucleus, the molecules situate at this limit ought, during the process of condensation, to separate from the rest of the atmospheric matter and form an equatorial zone, a ring revolving separately and with its primitive velocity. We may conceive that analogous separations were effected in the higher strata of the nebula at different epochs, that is to say, at different distances from the nucleus, and that they give rise to a succession of distinct rings, included almost in the same plane and endued with different velocities.

This being once admitted, it is easy to see that the indefinite stability of the rings would have required a regularity of structure throughout their whole contour, which is very improbable. Each of them accordingly broke in its turn into several masses, which were plainly endued with a movement of rotation, coinciding in direction with the common movement of revolution, and which in consequence of their fluidity assumed spheroidal forms.

In order, then, that one of those spheroids might absorb all the others belonging to the same ring, it will be sufficient to assign to it a mass greater than that of any other spheroid.

Each of the planets, while in the vaporous condition to which we have just alluded, would manifestly have a central nucleus gradually increasing in magnitude and mass, and an atmosphere offering, at its successive limits, phenomena entirely similar to those

which the solar atmosphere, properly so called, had exhibited. We here witness the birth of satellites, and that of the ring of Saturn.

The system, of which I have just given an imperfect sketch, has for its object to show how a nebula endued with a general movement of rotation must eventually transform itself into a very luminous central nucleus (a sun) and into a series of distinct spheroidal planets, situate at considerable distances from each other, revolving all around the central sun in the direction of the original movement of the nebula; how these planets ought also to have movements of rotation operating in similar directions; how, finally, the satellites, when any of such are formed, cannot fail to revolve upon their axes and around their respective primaries, in the direction of rotation of the planets and of their movement of revolution around the sun.

We have just found, conformably to the principles of mechanics, the forces with which the particles of the nebula were originally endued, in the movements of rotation and revolution of the compact and distinct masses which these particles have brought into existence by their condensation. But we have thereby achieved only a single step. The primitive movement of rotation of the nebula is not connected with the simple attraction of the particles. This movement seems to imply the action of a primordial impulsive force.

Laplace is far from adopting, in this respect, the almost universal opinion of philosophers and mathematicians. He does not suppose that the mutual attractions of originally immoveable bodies must ultimately reduce all the bodies to a state of rest around their common centre of gravity. He maintains, on the contrary, that three bodies, in a state of rest, two of which have a much greater mass than the third, would concentrate into a single mass only in certain exceptional cases. In general, the two most considerable bodies would unite together, while the third would revolve around their common centre of gravity. Attraction would thus become the cause of a sort of movement which would seem to be explicable solely by an impulsive force.

It might be supposed, indeed, that in explaining this part of his system Laplace had before his eyes the words which Rousseau has placed in the mouth of the vicar of Savoy, and that he wished to refute them: "Newton has discovered the law of attraction," says the author of *Emile*, "but attraction alone would soon reduce the universe to an immoveable mass: with this law we must combine a projectile force in order to make the celestial bodies describe curve lines. Let Descartes reveal to us the physical law which causes his vortices to revolve; and let Newton show us the hand which launched the planets along the tangents of their orbits."

According to the cosmogonic ideas of Laplace, comets did not

originally form part of the solar system; they are not formed at the expense of the matter of the immense solar nebula; we must consider them as small wandering nebulæ which the attractive force of the sun has caused to deviate from their original route. Such of those comets as penetrated into the great nebula at the epoch of condensation and of the formation of planets fell into the sun, describing spiral curves, and must by their action have caused the planetary orbits to deviate more or less from the plane of the solar equator, with which they would otherwise have exactly coincided.

With respect to the zodiacal light, that rock against which so many reveries have been wrecked, it consists of the most volatile parts of the primitive nebula. These molecules not having united with the equatorial zones successively abandoned in the plane of the solar equator, continue to revolve at their original distances, and with their original velocities. The circumstance of this extremely rare substance being included wholly within the earth's orbit, and even within that of Venus, seemed irreconcileable with the principles of mechanics; but this difficulty occurred only when the zodiacal substance being conceived to be in a state of direct and intimate dependence on the solar photosphere properly so called, an angular movement of rotation was impressed on it equal to that of the photosphere, a movement in virtue of which it effected an entire revolution in twenty-five days and a half. Laplace presented his conjectures on the formation of the solar system with the diffidence inspired by a result which was not founded upon calculation and observation.* Perhaps it is to be regretted that they did not receive a more complete development, especially in so far as concerns the division of the matter into distinct rings; perhaps it would have been desirable if the illustrious author had expressed himself more fully respecting the primitive physical condition, the molecular condition of the nebula at the expense of which the sun, planets, and satellites, of our system were formed. It is perhaps especially to be regretted that Laplace should have only briefly alluded to what he considered the obvious possibility of movements of revolution having their origin in the action of simple attractive forces, and to other questions of a similar nature.

Notwithstanding these defects, the ideas of the author of the *Mécanique Céleste* are still the only speculations of the kind which, by their magnitude, their coherence, and their mathematical character, may be justly considered as forming a physical cosmogony; those alone which in the present day derive a powerful

* Laplace has explained this theory in his *Exposition du Système du Monde* (liv. iv. note vii.). — *Translator.*

support from the results of the recent researches of astronomers on the nebulæ of every form and magnitude, which are scattered throughout the celestial vault.

In this analysis, we have deemed it right to concentrate all our attention upon the *Mécanique Céleste.* The *Système du Monde* and the *Théorie Analytique des Probabilités* would also require detailed notices.

The *Exposition du Système du Monde* is the *Mécanique Céleste* divested of the great apparatus of analytical formulæ which ought to be attentively perused by every astronomer who, to use an expression of Plato, is desirous of knowing the numbers which govern the physical universe. It is in the *Exposition du Système du Monde* that persons unacquainted with mathematical studies will obtain an exact and competent knowledge of the methods to which physical astronomy is indebted for its astonishing progress. This work, written with a noble simplicity of style, an exquisite propriety of expression, and a scrupulous accuracy, is terminated by a sketch of the history of astronomy, universally ranked in the present day among the finest monuments of the French language.

A regret has been often expressed, that Cæsar, in his immortal *Commentaries,* should have confined himself to a narration of his own campaigns: the astronomical commentaries of Laplace ascend to the origin of communities. The labours undertaken in all ages for the purpose of extracting new truths from the heavens, are there justly, clearly, and profoundly analysed; it is genius presiding as the impartial judge of genius. Laplace has always remained at the height of his great mission; his work will be read with respect so long as the torch of science shall continue to throw any light.

The calculus of probabilities, when confined within just limits, ought to interest, in an equal degree, the mathematician, the experimentalist, and the statesman. From the time when Pascal and Fermat established its first principles, it has rendered and continues daily to render services of the most eminent kind. It is the calculus of probabilities which, after having suggested the best arrangements of the tables of population and mortality, teaches us to deduce from those numbers, in general so erroneously interpreted, conclusions of a precise and useful character: it is the calculus of probabilities which alone can regulate justly the premiums to be paid for assurances; the reserve funds for the disbursement of pensions, annuities, discounts, &c.: it is under its influence that lotteries, and other shameful snares cunningly laid for avarice and ignorance, have definitively disappeared. Laplace has treated these questions, and others of a much more complicated nature, with his

accustomed superiority. In short, the *Théorie Analytique des Probabilités* is worthy of the author of the *Mécanique Céleste.*

A philosopher, whose name is associated with immortal discoveries, said to his audience who had allowed themselves to be influenced by ancient and consecrated authorities, "Bear in mind, Gentlemen, that in questions of science the authority of a thousand is not worth the humble reasoning of a single individual." Two centuries have passed over these words of Galileo without depreciating their value, or obliterating their truthful character. Thus, instead of displaying a long list of illustrious admirers of the three beautiful works of Laplace, we have preferred glancing briefly at some of the sublime truths which geometry has there deposited. Let us not, however, apply this principle in its utmost rigour, and since chance has put into our hands some unpublished letters of one of those men of genius, whom nature has endowed with the rare faculty of seizing at a glance the salient points of an object, we may be permitted to extract from them two or three brief and characteristic appreciations of the *Mécanique Céleste* and the *Traité des Probabilités.*

On the 27th Vendemiaire in the year X., General Bonaparte, after having received a volume of the *Mécanique Céleste,* wrote to Laplace in the following terms: "The first *six months* which I shall have at my disposal will be employed in reading your beautiful work." It would appear that the words, the first *six months,* deprive the phrase of the character of a common-place expression of thanks, and convey a just appreciation of the importance and difficulty of the subject matter.

On the 5th Frimaire in the year XI., the reading of some chapters of the volume, which Laplace had dedicated to him, was to the general "a new occasion for regretting, that the force of circumstances had directed him into a career which removed him from the pursuit of science."

"At all events," added he, "I have a strong desire that future generations, upon reading the *Mécanique Céleste,* shall not forget the esteem and friendship which I have entertained towards its author."

On the 17th Prairial in the year XIII., the general, now become emperor, wrote from Milan: "The *Mécanique Céleste* appears to me destined to shed new lustre on the age in which we live."

Finally, on the 12th of August, 1812, Napoleon, who had just received the *Traité du Calcul des Probabilités,* wrote from Witepsk the letter which we transcribe textually: —

"There was a time when I would have read with interest your *Traité du Calcul des Probabilités.* For the present I must confine myself to expressing to you the satisfaction which I experience

every time that I see you give to the world new works which serv to improve and extend the most important of the sciences, an contribute to the glory of the nation. The advancement and th improvement of mathematical science are connected with the pros perity of the state."

I have now arrived at the conclusion of the task which I ha imposed upon myself. I shall be pardoned for having given s detailed an exposition of the principal discoveries for which philc sophy, astronomy, and navigation, are indebted to our geometers.

It has appeared to me that in thus tracing the glorious past have shown our contemporaries the full extent of their duty tc wards the country. In fact, it is for nations especially to bea in remembrance the ancient adage: *noblesse oblige!*

APPENDIX.

(A.)

THE FOLLOWING IS A BRIEF NOTICE OF SOME OTHER INTERESTING RESULTS OF THE RESEARCHES OF LAPLACE WHICH HAVE NOT BEEN MENTIONED IN THE TEXT.

Method for determining the orbits of comets.—Since comets are generally visible only during a few days or weeks at the utmost, the determination of their orbits is attended with peculiar difficulties. The method devised by Newton for effecting this object was in every respect worthy of his genius. Its practical value was illustrated by the brilliant researches of Halley on cometary orbits. It necessitated, however, a long train of tedious calculations, and, in consequence, was not much used, astronomers generally preferring to attain the same end by a tentative process. In the year 1780, Laplace communicated to the Academy of Sciences an analytical method for determining the elements of a comet's orbit. This method has been extensively employed in France. Indeed, previously to the appearance of Olber's method, about the close of the last century, it furnished the easiest and most expeditious process hitherto devised, for calculating the parabolic elements of a comet's orbit.

Invariable plane of the solar system.—In consequence of the mutual perturbations of the different bodies of the planetary system, the planes of the orbits in which they revolve are perpetually varying in position. It becomes therefore desirable to ascertain some fixed plane to which the movements of the planets in all ages may be referred, so that the observations of one epoch might be rendered readily comparable with those of another. This object was accomplished by Laplace, who discovered that notwithstanding the perpetual fluctuations of the planetary orbits, there exists a fixed plane, to which the positions of the various bodies may at any instant be easily referred. This plane passes through the centre of gravity of the solar system, and its position is such, that if the movements of the planets be projected upon it, and if the mass of each planet be multiplied by the area which it describes in a given time, the sum of such products will be a maximum. The position of the plane for the year 1750 has been calculated by referring it to the ecliptic of that year. In this way it has been found that the inclination of the plane is 1° 35′ 31″, and that the longitude

of the ascending node is 102° 57′ 30″. The position of the plane when calculated for the year 1950, with respect to the ecliptic of 1750, gives 1° 35′ 31″ for the inclination, and 102° 57′ 15″ for the longitude of the ascending node. It will be seen that a very satisfactory accordance exists between the elements of the position of the invariable plane for the two epochs.

Diminution of the obliquity of the ecliptic.—The astronomers of the eighteenth century had found, by a comparison of ancient with modern observations, that the obliquity of the ecliptic is slowly diminishing from century to century. The researches of geometers on the theory of gravitation had shown that an effect of this kind must be produced by the disturbing action of the planets on the earth. Laplace determined the secular displacement of the plane of the earth's orbit due to each of the planets, and in this way ascertained the whole effect of perturbation upon the obliquity of the ecliptic. A comparison which he instituted between the results of his formula and an ancient observation recorded in the Chinese Annals exhibited a most satisfactory accordance. The observation in question indicated the obliquity of the ecliptic for the year 1100 before the Christian era, to be 23° 54′ 2″·5. According to the principles of the theory of gravitation, the obliquity for the same epoch would be 23° 51′ 30″.

Limits of the obliquity of the ecliptic modified by the action of the sun and moon upon the terrestrial spheroid.—The ecliptic will not continue indefinitely to approach the equator. After attaining a certain limit it will then vary in the opposite direction, and the obliquity will continually increase in like manner as it previously diminished. Finally, the inclination of the equator and the ecliptic will attain a certain maximum value, and then the obliquity will again diminish. Thus the angle contained between the two planes will perpetually oscillate within certain limits. The extent of variation is inconsiderable. Laplace found that, in consequence of the spheroidal figure of the earth, it is even less than it would otherwise have been. This will be readily understood, when we state that the disturbing action of the sun and moon upon the terrestrial spheroid produces an oscillation of the earth's axis which occasions a periodic variation of the obliquity of the ecliptic. Now, as the plane of the ecliptic approaches the equator, the mean disturbing action of the sun and moon upon the redundant matter accumulated around the latter will undergo a corresponding variation, and hence will arise an inconceivably slow movement of the plane of the equator, which will necessarily affect the obliquity of the ecliptic. Laplace found that if it were not for this cause, the obliquity of the ecliptic would oscillate to the extent of 4° 53′ 33″ on each side of a mean value, but that when the movements of both planes are taken into account, the extent of oscillation is reduced to 1° 33′ 45″.

Variation of the length of the tropical year.—The disturbing action of the sun and moon upon the terrestrial spheroid occasions a continual *regression* of the equinoctial points, and hence arises the distinction between the sidereal and tropical year. The effect is modified in a small degree by the variation of the plane of the ecliptic, which tends

to produce a *progression* of the equinoxes. If the movement of the equinoctial points arising from these combined causes was uniform, the length of the tropical year would be manifestly invariable. Theory, however, indicates that for ages past the rate of regression has been slowly increasing, and consequently, the length of the tropical year has been gradually diminishing. The rate of diminution is exceedingly small. Laplace found that it amounts to somewhat less than half a second in a century. Consequently, the length of the tropical year is now about ten seconds less than it was in the time of Hipparchus.

Limits of variation of the tropical year modified by the disturbing action of the sun and moon upon the terrestrial spheroid.— The tropical year will not continue indefinitely to diminish in length. When it has once attained a certain minimum value, it will then increase until finally having attained an extreme value in the opposite direction, it will again begin to diminish, and thus it will perpetually oscillate between certain fixed limits. Laplace found that the extent to which the tropical year is liable to vary from this cause, amounts to 38 seconds. If it were not for the effect produced upon the inclination of the equator to the ecliptic by the mean disturbing action of the sun and moon upon the terrestrial spheroid, the extent of variation would amount to 162 seconds.

Motion of the perihelion of the terrestrial orbit.— The major axis of the orbit of each planet is in a state of continual movement from the disturbing action of the other planets. In some cases, it makes the complete tour of the heavens; in others, it merely oscillates around a mean position. In the case of the earth's orbit, the perihelion is slowly advancing in the same direction as that in which all the planets are revolving around the sun. The alteration of its position with respect to the stars amounts to about 11″ in a year, but since the equinox is regressing in the opposite direction at the rate of 50″ in a year, the whole annual variation of the longitude of the terrestrial perihelion amounts to 61″. Laplace has considered two remarkable epochs in connexion with this fact; viz., the epoch at which the major axis of the earth's orbit coincided with the line of the equinoxes, and the epoch at which it stood perpendicular to that line. By calculation, he found the former of these epochs to be referable to the year 4107, B.C., and the latter to the year 1245, A.D. He accordingly suggested that the latter should be used as a universal epoch for the regulation of chronological occurrences.

(B.)

The *Mécanique Céleste.*— This stupendous monument of intellectual research consists, as stated by the author, of five quarto volumes. The subject-matter is divided into sixteen books, and each book again is subdivided into several chapters. Vol. i. contains the first and second books of the work; vol. ii. contains the third, fourth, and fifth books; vol. iii. contains the sixth and seventh books; vol. iv. contains the eighth, ninth, and tenth books; and, finally, vol. v. contains the re-

maining six books. In the first book the author treats of the general laws of equilibrium and motion. In the second book he treats of the law of gravitation, and the movements of the centres of gravity of the celestial bodies. In the third book he investigates the subject of the figures of the celestial bodies. In the fourth book he considers the oscillations of the ocean and the atmosphere, arising from the disturbing action of the celestial bodies. The fifth book is devoted to the investigation of the movements of the celestial bodies around their centres of gravity. In this book the author gives a solution of the great problems of the precession of the equinoxes and the libration of the moon, and determines the conditions upon which the stability of Saturn's ring depends. The sixth book is devoted to the theory of the planetary movements; the seventh, to the lunar theory; the eighth, to the theory of the satellites of Jupiter, Saturn, and Uranus; and the ninth, to the theory of comets. In the tenth book the author investigates various subjects relating to the system of the universe. Among these may be mentioned the theory of astronomical refractions; the determination of heights by the barometer; the investigation of the effects produced on the movements of the planets and comets by a resisting medium; and the determination of the values of the masses of the planets and satellites. In the six books forming the fifth volume of the work, the author, besides presenting his readers with an historical exposition of the labours of Newton and his successors on the theory of gravitation, gives an account of various researches relative to the system of the universe, which had occupied his attention subsequently to the publication of the previous volumes. In the eleventh book he considers the subjects of the figure and rotation of the earth. In the twelfth book he investigates the attraction and repulsion of spheres, and the laws of equilibrium and motion of elastic fluids. The thirteenth book is devoted to researches on the oscillations of the fluids which cover the surfaces of the planets; the fourteenth, to the subject of the movements of the celestial bodies around their centres of gravity; the fifteenth, to the movements of the planets and comets; and the sixteenth, to the movements of the satellites. The author published a supplement to the third volume, containing the results of certain researches on the planetary theory, and a supplement to the tenth book, in which he investigates very fully the theory of capillary attraction. There was also published a posthumous supplement to the fifth volume, the manuscript of which was found among his papers after his death.

JOSEPH FOURIER.

BIOGRAPHY READ AT A PUBLIC ASSEMBLY OF THE ACADEMY OF SCIENCES, ON THE 18TH OF NOVEMBER, 1833.

GENTLEMEN, — In former times one academician differed from another only in the number, the nature, and the brilliancy of his discoveries. Their lives, thrown in some respects into the same mould, consisted of events little worthy of remark. A boyhood more or less studious; progress sometimes slow, sometimes rapid; inclinations thwarted by capricious or shortsighted parents; inadequacy of means, the privations which it introduces in its train; thirty years of a laborious professorship and difficult studies, — such were the elements from which the admirable talents of the early secretaries of the Academy were enabled to execute those portraits, so piquant, so lively, and so varied, which form one of the principal ornaments of your learned collections.

In the present day, biographies are less confined in their object. The convulsions which France has experienced in emancipating herself from the swaddling clothes of routine, of superstition and of privilege, have cast into the storms of political life citizens of all ages, of all conditions, and of all characters. Thus has the Academy of Sciences figured during forty years in the devouring arena, wherein might and right have alternately seized the supreme power by a glorious sacrifice of combatants and victims!

Recall to mind, for example, the immortal National Assembly. You will find at its head a modest academician, a pattern of all the private virtues, the unfortunate Bailly, who, in the different phases of his political life, knew how to reconcile a passionate affection for his country with a moderation which his most cruel enemies themselves have been compelled to admire.

When, at a later period, coalesced Europe launched against France a million of soldiers; when it became necessary to organise for the crisis fourteen armies, it was the ingenious author of the *Essai sur les Machines* and of the *Géométrie des Positions* who

directed this gigantic operation. It was, again, Carnot, our honourable colleague, who presided over the incomparable campaign of seventeen months, during which French troops, novices in the profession of arms, gained eight pitched battles, were victorious in one hundred and forty combats, occupied one hundred and sixteen fortified places and two hundred and thirty forts or redoubts, enriched our arsenals with four thousand cannon and seventy thousand muskets, took a hundred thousand prisoners, and adorned the dome of the Invalides with ninety flags. During the same time the Chaptals, the Fourcroys, the Monges, the Berthollets rushed also to the defence of French independence, some of them extracting from our soil, by prodigies of industry, the very last atoms of saltpetre which it contained; others transforming, by the aid of new and rapid methods, the bells of the towns, villages, and smallest hamlets into a formidable artillery, which our enemies supposed, as indeed they had a right to suppose, we were deprived of. At the voice of his country in danger, another academician, the young and learned Meunier, readily renounced the seductive pursuits of the laboratory: he went to distinguish himself upon the ramparts of Königstein, to contribute as a hero to the long defence of Mayence, and met his death, at the age of forty years only, after having attained the highest position in a garrison wherein shone the Aubert-Dubayets, the Beaupuys, the Haxos, the Klebers.

How could I forget here the last secretary of the original Academy? Follow him into a celebrated assembly, into that Convention, the sanguinary delirium of which we might almost be inclined to pardon, when we call to mind how gloriously terrible it was to the enemies of our independence, and you will always see the illustrious Condorcet occupied exclusively with the great interests of reason and humanity. You will hear him denounce the shameful brigandage which for two centuries laid waste the African continent by a system of corruption; demand in a tone of profound conviction that the Code be purified of the frightful stain of capital punishment, which renders the error of the judge for ever irreparable. He is the official organ of the Assembly on every occasion when it is necessary to address soldiers, citizens, political parties, or foreign nations in language worthy of France; he is not the tactician of any party, he incessantly entreats all of them to occupy their attention less with their own interests and a little more with public matters; he replies, finally, to unjust reproaches of weakness by acts which leave him the only alternative of the poison cup or the scaffold.

The French Revolution thus threw the learned geometer whose discoveries I am about to celebrate far away from the route which

destiny appeared to have traced out for him. In ordinary times it would be about Dom * Joseph Fourier that the secretary of the Academy would have deemed it his duty to have occupied your attention. It would be the tranquil, the retired life of a Benedictine which he would have unfolded to you. The life of our colleague, on the contrary, will be agitated and full of perils; it will pass into the fierce contentions of the forum and amid the hazards of war; it will be a prey to all the anxieties which accompany a difficult administration. We shall find this life intimately associated with the great events of our age. Let us hasten to add, that it will be always worthy and honourable, and that the personal qualities of the man of science will enhance the brilliancy of his discoveries.

BIRTH OF FOURIER. — HIS YOUTH.

Fourier was born at Auxerre on the 21st of March, 1768. His father, like that of the illustrious geometer Lambert, was a tailor. This circumstance would formerly have occupied a large place in the *éloge* of our learned colleague; thanks to the progress of enlightened ideas, I may mention the circumstance as a fact of no importance: nobody, in effect, thinks in the present day, nobody even pretends to think, that genius is the privilege of rank or fortune.

Fourier became an orphan at the age of eight years. A lady who had remarked the amiability of his manners and his precocious natural abilities, recommended him to the Bishop of Auxerre. Through the influence of this prelate, Fourier was admitted into the military school which was conducted at that time by the Benedictines of the Convent of St. Mark. There he prosecuted his literary studies with surprising rapidity and success. Many sermons very much applauded at Paris in the mouth of high dignitaries of the Church were emanations from the pen of the schoolboy of twelve years of age. It would be impossible in the present day to trace those first compositions of the youth Fourier, since, while divulging the plagiarism, he had the discretion never to name those who profited by it.

At thirteen years Fourier had the petulance, the noisy vivacity of most young people of the same age; but his character changed all at once, and as if by enchantment, as soon as he was initiated in the first principles of mathematics, that is to say, as soon as he became sensible of his real vocation. The hours prescribed for study no longer sufficed to gratify his insatiable curiosity. Ends

* An abbreviation of Dominus, equivalent to the English prefix Reverend.—*Translator.*

of candles carefully collected in the kitchen, the corridors and the refectory of the college, and placed on a hearth concealed by a screen, served during the night to illuminate the solitary studies by which Fourier prepared himself for those labours which were destined, a few years afterwards, to adorn his name and his country.

In a military school directed by monks, the minds of the pupils necessarily waver only between two careers in life — the church and the sword. Like Descartes, Fourier wished to be a soldier; like that philosopher, he would doubtless have found the life of a garrison very wearisome. But he was not permitted to make the experiment. His demand to undergo the examination for the artillery, although strongly supported by our illustrious colleague Legendre, was rejected with a severity of expression of which you may judge yourselves: "Fourier," replied the minister, "not being noble, could not enter the artillery, although he were a second Newton."

Gentlemen, there is in the strict enforcement of regulations, even when they are most absurd, something respectable which I have a pleasure in recognising; in the present instance nothing could soften the odious character of the minister's words. It is not true in reality that no one could formerly enter into the artillery who did not possess a title of nobility: a certain fortune frequently supplied the want of parchments. Thus it was not a something undefinable, which, by the way, our ancestors the Franks had not yet invented, that was wanting to young Fourier, but rather an income of a few hundred livres, which the men who were then placed at the head of the country would have refused to acknowledge the genius of Newton as a just equivalent for! Treasure up these facts, Gentlemen; they form an admirable illustration of the immense advances which France has made during the last forty years. Posterity, moreover, will see in this, not the excuse, but the explanation of some of those sanguinary dissensions which stained our first revolution.

Fourier, not having been enabled to gird on the sword, assumed the habit of a Benedictine, and repaired to the Abbey of St. Benoît-sur-Loire, where he intended to pass the period of his noviciate. He had not yet taken any vows when, in 1789, every mind was captivated with beautifully seductive ideas relative to the social regeneration of France. Fourier now renounced the profession of the Church; but this circumstance did not prevent his former masters from appointing him to the principal chair of mathematics in the Military School of Auxerre, and bestowing upon him numerous tokens of a lively and sincere affection. I venture to assert that no event in the life of our colleague affords a more striking

proof of the goodness of his natural disposition and the amiability of his manners. It would be necessary not to know the human heart to suppose that the monks of St. Benoît did not feel some chagrin upon finding themselves so abruptly abandoned, to imagine especially that they should give up without lively regret the glory which the order might have expected from the ingenious colleague who had just escaped from them.

Fourier responded worthily to the confidence of which he had just become the object. When his colleagues were indisposed, the titular professor of mathematics occupied in turns the chairs of rhetoric, of history, and of philosophy; and whatever might be the subject of his lectures, he diffused among an audience which listened to him with delight, the treasures of a varied and profound erudition, adorned with all the brilliancy which the most elegant diction could impart to them.

MEMOIR ON THE RESOLUTION OF NUMERICAL EQUATIONS.

About the close of the year 1789 Fourier repaired to Paris and read before the Academy of Sciences a memoir on the resolution of numerical equations of all degrees. This work of his early youth our colleague, so to speak, never lost sight of. He explained it at Paris to the pupils of the Polytechnic School; he developed it upon the banks of the Nile in presence of the Institute of Egypt; at Grenoble, from the year 1802, it was his favourite subject of conversation with the Professors of the Central School and of the Faculty of Sciences: this finally, contained the elements of the work which Fourier was engaged in seeing through the press when death put an end to his career.

A scientific subject does not occupy so much space in the life of a man of science of the first rank without being important and difficult. The subject of algebraic analysis above mentioned, which Fourier had studied with a perseverance so remarkable, is not an exception to this rule. It offers itself in a great number of applications of calculation to the movements of the heavenly bodies, or to the physics of terrestrial bodies, and in general in the problems which lead to equations of a high degree. As soon as he wishes to quit the domain of abstract relations, the calculator has occasion to employ the roots of these equations; thus the art of discovering them by the aid of an uniform method, either exactly or by approximation, did not fail at an early period to excite the attention of geometers.

An observant eye perceives already some traces of their efforts in the writings of the mathematicians of the Alexandrian School. These traces, it must be *acknowledged*, are so slight and so imper-

fect, that we should truly be justified in referring the origin of this branch of analysis only to the excellent labours of our countryman Vieta. Descartes, to whom we render very imperfect justice when we content ourselves with saying that he taught us much when he taught us to doubt, occupied his attention also for a short time with this problem, and left upon it the indelible impress of his powerful mind. Hudde gave for a particular but very important case rules to which nothing has since been added; Rolle, of the Academy of Sciences, devoted to this one subject his entire life. Among our neighbours on the other side of the channel, Harriot, Newton, Maclaurin, Stirling, Waring, I may say all the illustrious geometers which England produced in the last century, made it also the subject of their researches. Some years afterwards the names of Daniel Barnoulli, of Euler, and of Fontaine came to be added to so many great names. Finally, Lagrange in his turn embarked in the same career, and at the very commencement of his researches he succeeded in substituting for the imperfect, although very ingenious, essays of his predecessors, a complete method which was free from every objection. From that instant the dignity of science was satisfied; but in such a case it would not be permitted to say with the poet:

"Le temps ne fait rien à l'affaire."

Now although the processes invented by Lagrange, simple in principle and applicable to every case, have theoretically the merit of leading to the result with certainty, still, on the other hand, they demand calculations of a most repulsive length. It remained then to perfect the practical part of the question: it was necessary to devise the means of shortening the route without depriving it in any degree of its certainty. Such was the principal object of the researches of Fourier, and this he has attained to a great extent.

Descartes had already found, in the order according to which the signs of the different terms of any numerical equation whatever succeed each other, the means of deciding, for example, how many real positive roots this equation may have. Fourier advanced a step further: he discovered a method for determining what number of the equally positive roots of every equation may be found included between two given quantities. Here certain calculations become necessary, but they are very simple, and whatever be the precision desired, they lead without any trouble to the solutions sought for.

I doubt whether it were possible to cite a single scientific discovery of any importance which has not excited discussions of priority. The new method of Fourier for solving numerical equations is in this respect amply comprised within the common

law. We ought, however, to acknowledge that the theorem which serves as the basis of this method, was first published by M. Budan; that according to a rule which the principal Academies of Europe have solemnly sanctioned, and from which the historian of the sciences dares not deviate without falling into arbitrary assumptions and confusion, M. Budan ought to be considered as the inventor. I will assert with equal assurance that it would be impossible to refuse to Fourier the merit of having attained the same object by his own efforts. I even regret that, in order to establish rights which nobody has contested, he deemed it necessary to have recourse to the certificates of early pupils of the Polytechnic School, or Professors of the University. Since our colleague had the modesty to suppose that his simple declaration would not be sufficient, why (and the argument would have had much weight,) did he not remark in what respect his demonstration differed from that of his competitor?—an admirable demonstration, in effect, and one so impregnated with the elements of the question, that a young geometer, M. Sturm, has just employed it to establish the truth of the beautiful theorem by the aid of which he determines not the simple limits, but the exact number of roots of any equation whatever which are comprised between two given quantities.

PART PLAYED BY FOURIER IN OUR REVOLUTION.—HIS ENTRANCE INTO THE CORPS OF PROFESSORS OF THE NORMAL SCHOOL AND THE POLYTECHNIC SCHOOL.—EXPEDITION TO EGYPT.

We had just left Fourier at Paris, submitting to the Academy of Sciences the analytical memoir of which I have just given a general view. Upon his return to Auxerre, the young geometer found the town, the surrounding country, and even the school to which he belonged, occupied intensely with the great questions relative to the dignity of human nature, philosophy, and politics, which were then discussed by the orators of the different parties of the National Assembly. Fourier abandoned himself also to this movement of the human mind. He embraced with enthusiasm the principles of the Revolution, and he ardently associated himself with everything grand, just, and generous which the popular impulse offered. His patriotism made him accept the most difficult missions. We may assert, that never, even when his life was at stake, did he truckle to the base, covetous, and sanguinary passions which displayed themselves on all sides.

A member of the popular society of Auxerre, Fourier exercised there an almost irresistible ascendency. One day—all Burgundy has preserved the remembrance of it—on the occasion of a levy of three hundred thousand men, he made the words honour, country,

glory, ring so eloquently, he induced so many voluntary enrolments, that the ballot was not deemed necessary. At the command of the orator the contingent assigned to the chief town of the Yonne formed in order, assembled together within the very enclosure of the Assembly, and marched forthwith to the frontier. Unfortunately these struggles of the forum, in which so many noble lives then exercised themselves, were far from having always a real importance. Ridiculous, absurd, and burlesque motions injured incessantly the inspirations of a pure, sincere, and enlightened patriotism. The popular society of Auxerre would furnish us, in case of necessity, with more than one example of those lamentable contrasts. Thus I might say that in the very same apartment wherein Fourier knew how to excite the honourable sentiments which I have with pleasure recalled to mind, he had on another occasion to contend with a certain orator, perhaps of good intentions, but assuredly a bad astronomer, who, wishing to escape, said he, from *the good pleasure* of municipal rulers, proposed that the names of the north, east, south, and west quarters should be assigned by lot to the different parts of the town of Auxerre.

Literature, the fine arts, and the sciences appeared for a moment to flourish under the auspicious influence of the French Revolution. Observe, for example, with what grandeur of conception the reformation of weights and measures was planned: what geometers, what astronomers, what eminent philosophers presided over every department of this noble undertaking! Alas! frightful revolutions in the interior of the country soon saddened this magnificent spectacle. The sciences could not prosper in the midst of the desperate contest of factions. They would have blushed to owe any obligations to the men of blood, whose blind passions immolated a Saron, a Bailly, and a Lavoisière.

A few months after the 9th Thermidor, the Convention being desirous of diffusing throughout the country ideas of order, civilisation, and internal prosperity, resolved upon organising a system of public instruction, but a difficulty arose in finding professors. The members of the corps of instruction had become officers of artillery, of engineering, or of the staff, and were combating the enemies of France at the frontiers. Fortunately at this epoch of intellectual exaltation, nothing seemed impossible. Professors were wanting; it was resolved without delay to create some, and the Normal School sprung into existence. Fifteen hundred citizens of all ages, despatched from the principal district towns, assembled together, not to study in all their ramifications the different branches of human knowledge, but in order to learn the art of teaching under the greatest masters.

Fourier was one of these fifteen hundred pupils. It will no

doubt, excite some surprise that he was elected at St. Florentine, and that Auxerre appeared insensible to the honour of being represented at Paris by the most illustrious of her children. But this indifference will be readily understood. The elaborate scaffolding of calumny which it has served to support will fall to the ground as soon as I recall to mind, that after the 9th Thermidor the capital, and especially the provinces, became a prey to a blind and disorderly reaction, as all political reactions invariably are; that crime (the crime of having changed opinions—it was nothing less hideous) usurped the place of justice; that excellent citizens, that pure, moderate, and conscientious patriots were daily massacred by hired bands of assassins in presence of whom the inhabitants remained mute with fear. Such are, Gentlemen, the formidable influences which for a moment deprived Fourier of the suffrages of his countrymen; and caricatured, as a partisan of Robespierre, the individual whom St. Just, making allusion to his sweet and persuasive eloquence, styled a *patriot in music;* who was so often thrown into prison by the decemvirs; who, at the very height of the Reign of Terror, offered before the revolutionary Tribunal the assistance of his admirable talents to the mother of Marshal Davoust, accused of the crime of having at that unrelenting epoch sent some money to the emigrants; who had the incredible boldness to shut up at the inn of Tonnerre an agent of the Committee of Public Safety, into the secret of whose mission he penetrated, and thus obtained time to warn an honourable citizen that he was about to be arrested; who, finally, attaching himself personally to the sanguinary proconsul before whom every one trembled in Yonne, made him pass for a madman, and obtained his recall! You see, Gentlemen, some of the acts of patriotism, of devotion, and of humanity which signalised the early years of Fourier. They were, you have seen, repaid with ingratitude. But ought we in reality to be astonished at it? To expect gratitude from the man who cannot make an avowal of his feelings without danger, would be to shut one's eyes to the frailty of human nature, and to expose one's self to frequent disappointments.

In the Normal School of the Convention, discussion from time to time succeeded ordinary lectures. On those days an interchange of characters was effected: the pupils interrogated the professors. Some words pronounced by Fourier at one of those curious and useful meetings sufficed to attract attention towards him. Accordingly, as soon as a necessity was felt to create Masters of Conference, all eyes were turned towards the pupil of St. Florentine. The precision, the clearness, and the elegance of his lectures soon procured for him the unanimous applause of the fastidious and numerous audience which was confided to him.

When he attained the height of his scientific and literary glory,

Fourier used to look back with pleasure upon the year 1794, and upon the sublime efforts which the French nation then made for the purpose of organising a Corps of Public Instruction. If he had ventured, the title of Pupil of the original Normal School would have been beyond doubt that which he would have assumed by way of preference. Gentlemen, that school perished of cold, of wretchedness, and of hunger, and not, whatever people may say, from certain defects of organisation which time and reflection would have easily rectified. Notwithstanding its short existence, it imparted to scientific studies quite a new direction which has been productive of the most important results. In supporting this opinion at some length, I shall acquit myself of a task which Fourier would certainly have imposed upon me, if he could have suspected, that with just and eloquent eulogiums of his character and his labours there should mingle within the walls of this apartment, and even emanate from the mouth of one of his successors, sharp critiques of his beloved Normal School.

It is to the Normal School that we must inevitably ascend if we would desire to ascertain the earliest public teaching of *descriptive Geometry*, that fine creation of the genius of Monge. It is from this source that it has passed almost without modification to the Polytechnic School, to foundries, to manufactories, and the most humble workshops.

The establishment of the Normal School accordingly indicates the commencement of a veritable revolution in the study of pure mathematics; with it demonstrations, methods, and important theories, buried in academical collections, appeared for the first time before the pupils, and encouraged them to recast upon new bases the works destined for instruction.

With some rare exceptions, the philosophers engaged in the cultivation of science constituted formerly in France a class totally distinct from that of the professors. By appointing the first geometers, the first philosophers, and the first naturalists of the world to be professors, the Convention threw new lustre upon the profession of teaching, the advantageous influence of which is felt in the present day. In the opinion of the public at large, a title which a Lagrange, a Laplace, a Monge, a Berthollet, had borne, became a proper match to the finest titles. If under the empire, the Polytechnic School counted among its active professors councillors of state, ministers, and the president of the senate, you must look for the explanation of this fact in the impulse given by the Normal School.

You see in the ancient great colleges, professors concealed in some degree behind their portfolios, reading as from a pulpit, amid the indifference and inattention of their pupils, discourses prepared beforehand with great labour, and which reappear every

year in the same form. Nothing of this kind existed at the Normal School: oral lessons alone were there permitted. The authorities even went so far as to require of the illustrious savans appointed to the task of instruction the formal promise never to recite any lectures which they might have learned by heart. From that time the chair has become a tribune where the professor, identified, so to speak, with his audience, sees in their looks, in their gestures, in their countenance, sometimes the necessity for proceeding at greater speed, sometimes, on the contrary, the necessity of retracing his steps, of awakening the attention by some incidental observations, of clothing in a new form the thought which, when first expressed, had left some doubts in the minds of his audience. And do not suppose that the beautiful impromptu lectures with which the amphitheatre of the Normal School resounded, remained unknown to the public. Short-hand writers paid by the State reported them. The sheets, after being revised by the professors, were sent to the fifteen hundred pupils, to the members of Convention, to the consuls and agents of the Republic in foreign countries, to all governors of districts. There was in this something certainly of profusion compared with the parsimonious and mean habits of our time. Nobody, however, would concur in this reproach, however slight it may appear, if I were permitted to point out in this very apartment an illustrious Academician, whose mathematical genius was awakened by the lectures of the Normal School in an obscure district town!

The necessity of demonstrating the important services, ignored in the present day, for which the dissemination of the sciences is indebted to the first Normal School, has induced me to dwell at greater length on the subject than I intended. I hope to be pardoned; the example in any case will not be contagious. Eulogiums of the past, you know, Gentlemen, are no longer fashionable. Everything which is said, everything which is printed, induces us to suppose that the world is the creation of yesterday. This opinion, which allows to each a part more or less brilliant in the cosmogonic drama, is under the safeguard of too many vanities to have anything to fear from the efforts of logic.

I have already stated that the brilliant success of Fourier at the Normal School assigned to him a distinguished place among the persons whom nature has endowed in the highest degree with the talent of public tuition. Accordingly, he was not forgotten by the founders of the Polytechnic School. Attached to that celebrated establishment, first with the title of Superintendent of Lectures on Fortification, afterwards appointed to deliver a course of lectures on analysis, Fourier has left there a venerated name, and the reputation of a professor distinguished by clearness, method, and erudition;

I shall add even the reputation of a professor full of grace, for our colleague has proved that this kind of merit may not be foreign to the teaching of mathematics.

The lectures of Fourier have not been collected together. The Journal of the Polytechnic School contains only one paper by him, a memoir upon the "principle of virtual velocities." This memoir, which probably had served for the text of a lecture, shows that the secret of our celebrated professor's great success consisted in the combination of abstract truths, of interesting applications, and of historical details little known, and derived, a thing so rare in our days, from original sources.

We have now arrived at the epoch when the peace of Leoben brought back to the metropolis the principal ornaments of our armies. Then the professors and the pupils of the Polytechnic School had sometimes the distinguished honour of sitting in their amphitheatres beside Generals Desaix and Bonaparte. Everything indicated to them then an active participation in the events which each foresaw, and which in fact were not long of occurring.

Notwithstanding the precarious condition of Europe, the Directory decided upon denuding the country of its best troops, and launching them upon an adventurous expedition. The five chiefs of the Republic were then desirous of removing from Paris the conqueror of Italy, of thereby putting an end to the popular demonstrations of which he everywhere formed the object, and which sooner or later would become a real danger.

On the other hand, the illustrious general did not dream merely of the momentary conquest of Egypt; he wished to restore to that country its ancient splendour; he wished to extend its cultivation, to improve its system of irrigation, to create new branches of industry, to open to commerce numerous outlets, to stretch out a helping hand to the unfortunate inhabitants, to rescue them from the galling yoke under which they had groaned for ages, in a word, to bestow upon them without delay all the benefits of European civilisation. Designs of such magnitude could not have been accomplished with the mere *personnel* of an ordinary army. It was necessary to appeal to science, to literature, and to the fine arts; it was necessary to ask the co-operation of several men of judgment and of experience. Monge and Berthollet, both members of the Institute and Professors in the Polytechnic School, became, with a view to this object, the principal recruiting aids to the chief of the expedition. Were our colleagues really acquainted with the object of this expedition? I dare not reply in the affirmative; but I know at all events that they were not permitted to divulge it. We are going to a distant country; we shall embark at Toulon; we shall be constantly with you; Ge-

neral Bonaparte will command the army, such was in form and substance the limited amount of confidential information which had been imperiously traced out to them. Upon the faith of words so vague, with the chances of a naval battle, with the English hulks in perspective, go in the present day and endeavour to enrol a father of a family, a savant already known by useful labours and placed in some honourable position, an artist in possession of the esteem and confidence of the public, and I am much mistaken if you obtain anything else than refusals; but in 1798, France had hardly emerged from a terrible crisis, during which her very existence was frequently at stake. Who, besides, had not encountered imminent personal danger? Who had not seen with his own eyes enterprises of a truly desperate nature brought to a fortunate issue? Is anything more wanted to explain that adventurous character, that absence of all care for the morrow, which appears to have been one of the most distinguishing features of the epoch of the Directory. Fourier accepted then without hesitation the proposals which his colleagues brought to him in the name of the Commander-in-Chief; he quitted the agreeable duties of a professor of the Polytechnic School, to go — he knew not where, to do — he knew not what.

Chance placed Fourier during the voyage in the vessel in which Kleber sailed. The friendship which the philosopher and the warrior vowed to each other from that moment was not without some influence upon the events of which Egypt was the theatre after the departure of Napoleon.

He who signed his orders of the day, the *Member of the Institute, Commander-in-Chief of the Army in the East,* could not fail to place an Academy among the means of regenerating the ancient kingdom of the Pharaohs. The valiant army which he commanded had barely conquered at Cairo, on the occasion of the memorable battle of the Pyramids, when the Institute of Egypt sprung into existence. It consisted of forty-eight members, divided into four sections. Monge had the honour of being the first president. As at Paris, Bonaparte belonged to the section of Mathematics. The situation of perpetual secretary, the filling up of which was left to the free choice of the Society, was unanimously assigned to Fourier.

You have seen the celebrated geometer discharge the same duty at the Academy of Sciences; you have appreciated his liberality of mind, his enlightened benevolence, his unvarying affability, his straightforward and conciliatory disposition: add in imagination to so many rare qualities the activity which youth, which health can alone give, and you will have again conjured into existence the Secretary of the Institute of Egypt; and yet the portrait

which I have attempted to draw of him would grow pale beside the original.

Upon the banks of the Nile, Fourier devoted himself to assiduous researches on almost every branch of knowledge which the vast plan of the Institute embraced. The *Decade* and the *Courier of Egypt* will acquaint the reader with the titles of his different labours. I find in these journals a memoir upon the general solution of algebraic equations; researches on the methods of elimination; the demonstration of a new theorem of algebra; a memoir upon the indeterminate analysis; studies on general mechanics; a technical and historical work upon the aqueduct which conveys the waters of the Nile to the Castle of Cairo; reflections upon the Oases; the plan of statistical researches to be undertaken with respect to the state of Egypt; programme of an intended exploration of the site of the ancient Memphis, and of the whole extent of burying places; a descriptive account of the revolutions and manners of Egypt, from the time of its conquest by Selim.

I find also in the Egyptian *Decade*, that, on the first complementary day of the year VI., Fourier communicated to the Institute the description of a machine designed to promote irrigation, and which was to be driven by the power of wind.

This work, so far removed from the ordinary current of the ideas of our colleague, has not been printed. It would very naturally find a place in a work of which the Expedition to Egypt might again furnish the subject, notwithstanding the many beautiful publications which it has already called into existence. It would be a description of the manufactories of steel, of arms, of powder, of cloth, of machines, and of instruments of every kind which our army had to prepare for the occasion. If, during our infancy, the expedients which Robinson Crusoe practised in order to escape from the romantic dangers which he had incessantly to encounter, excite our interest in a lively degree, how, in mature age, could we regard with indifference a handful of Frenchmen thrown upon the inhospitable shores of Africa, without any possible communication with the mother country, obliged to contend at once with the elements and with formidable armies, destitute of food, of clothing, of arms and of ammunition, and yet supplying every want by the force of genius!

The long route which I have yet to traverse, will hardly allow me to add a few words relative to the administrative services of the illustrious geometer. Appointed French Commissioner at the Divan of Cairo, he became the official medium between the General-in-Chief and every Egyptian who might have to complain of an attack against his person, his property, his morals, his habits, or his creed. An invariable suavity of manner, a scru-

pulous regard for prejudices to oppose which directly would have been vain, an inflexible sentiment of justice, had given him an ascendency over the Mussulman population, which the precepts of the Koran could not lead any one to hope for, and which powerfully contributed to the maintenance of friendly relations between the inhabitants of Cairo and the French soldiers. Fourier was especially held in veneration by the Cheiks and the Ulemas. A single anecdote will serve to show that this sentiment was the offspring of genuine gratitude.

The Emir Hadgey, or Prince of the Caravan, who had been nominated by General Bonaparte upon his arrival in Cairo, escaped during the campaign of Syria. There existed strong grounds at the time for supposing that four *Cheiks Ulémas* had rendered themselves accomplices of the treason. Upon his return to Egypt, Bonaparte confided the investigation of this grave affair to Fourier. "Do not," said he, "submit half measures to me. You have to pronounce judgment upon high personages: we must either cut off their heads or invite them to dinner." On the day following that on which this conversation took place, the four Cheiks dined with the General-in-Chief. By obeying the inspirations of his heart, Fourier did not perform merely an act of humanity; it was moreover one of excellent policy. Our learned colleague, M. Geoffry Saint-Hilaire, to whom I am indebted for this anecdote, has stated in fact that Soleyman and Fayoumi, the principal of the Egyptian chiefs, whose punishment, thanks to our colleague, was so happily transformed into a banquet, seized every occasion of extolling among their countrymen the generosity of the French.

Fourier did not display less ability when our generals confided diplomatic missions to him. It is to his tact and urbanity that our army is indebted for an offensive and defensive treaty of alliance with Mourad Bey. Justly proud of this result, Fourier omitted to make known the details of the negotiation. This is deeply to be regretted, for the plenipotentiary of Mourad was a woman, the same Sitty Neficah whom Kleber has immortalised by proclaiming her *beneficence, her noble character,* in the bulletin of Heliopolis, and who moreover was already celebrated from one extremity of Asia to the other, in consequence of the bloody revolutions which her unparalleled beauty had excited among the Mamelukes.

The incomparable victory which Kleber gained over the army of the Grand Vizier did not damp the energy of the Janissaries, who had seized upon Cairo while the war was raging at Heliopolis. They defended themselves from house to house with heroic courage. The besieged had to choose between the entire destruction of the city and an honourable capitulation. The latter alternative was

adopted. Fourier, charged, as usual, with the negotiations, conducted them to a favourable issue; but on this occasion the treaty was not discussed, agreed to, and signed within the mysterious precincts of a harem, upon downy couches, under the shade of balmy groves. The preliminary discussions were held in a house half ruined by bullets and grape-shot; in the centre of the quarter of which the insurgents valiantly disputed the possession with our soldiers; before even it would have been possible to agree to the basis of a treaty of a few hours. Accordingly, when Fourier was preparing to celebrate the welcome of the Turkish commissioner conformably to oriental usages, a great number of musket-shots were fired from the house in front, and a ball passed through the coffee-pot which he was holding in his hand. Without calling in question the bravery of any person, do you not think, Gentlemen, that if diplomatists were usually placed in equally perilous positions, the public would have less reason to complain of their proverbial slowness?

In order to exhibit, under one point of view, the various administrative duties of our indefatigable colleague, I should have to show him to you on board the English fleet, at the instant of the capitulation of Menou, stipulating for certain guarantees in favour of the members of the Institute of Egypt; but services of no less importance and of a different nature demand also our attention. They will even compel us to retrace our steps, to ascend even to the epoch of glorious memory when Desaix achieved the conquest of Upper Egypt, as much by the sagacity, the moderation, and the inflexible justice of all his acts, as by the rapidity and boldness of his military operations. Bonaparte then appointed two numerous commissions to proceed to explore in those remote regions, a multitude of monuments of which the moderns hardly suspected the existence. Fourier and Costas were the commandants of these commissions; I say the commandants, for a sufficiently imposing military force had been assigned to them; since it was frequently after a combat with the wandering tribes of Arabs that the astronomer found in the movements of the heavenly bodies the elements of a future geographical map; that the naturalist collected unknown plants, determined the geological constitution of the soil, occupied himself with troublesome dissections; that the antiquary measured the dimensions of edifices, that he attempted to take a faithful sketch of the fantastic images with which everything was covered in that singular country, — from the smallest pieces of furniture, from the simple toys of children, to those prodigious palaces, to those immense façades, beside which the vastest of modern constructions would hardly attract a look.

The two learned commissions studied with scrupulous care the magnificent temple of the ancient Tentyris, and especially the series of astronomical signs which have excited in our days such lively discussions; the remarkable monuments of the mysterious and sacred Isle of Elephantine; the ruins of Thebes with her hundred gates, before which (and yet they are nothing but ruins) our whole army halted, in a state of astonishment, to applaud.

Fourier also presided in Upper Egypt over these memorable works, when the Commander-in-Chief suddenly quitted Alexandria and returned to France with his principal friends. Those persons then were very much mistaken who, upon not finding our colleague on board the frigate *Muiron* beside Monge and Berthollet, imagined that Bonaparte did not appreciate his eminent qualities. If Fourier was not a passenger, this arose from the circumstance of his having been a hundred leagues from the Mediterranean when the *Muiron* set sail. The explanation contains nothing striking, but it is true. In any case, the friendly feeling of Kleber towards the Secretary of the Institute of Egypt, the influence which he justly granted to him on a multitude of delicate occasions, amply compensated him for an unjust omission.

I arrive, Gentlemen, at the epoch so suggestive of painful recollections, when the *Agas* of the Janissaries who had fled into Syria, having despaired of vanquishing our troops so admirably commanded, by the honourable arms of the soldier, had recourse to the dagger of the assassin. You are aware that a young fanatic, whose imagination had been wrought up to a high state of excitement in the mosques by a month of prayers and abstinence, aimed a mortal blow at the hero of Heliopolis at the instant when he was listening, without suspicion, and with his usual kindness, to a recital of pretended grievances and was promising redress.

This sad misfortune plunged our colony into profound grief. The Egyptians themselves mingled their tears with those of the French soldiers. By a delicacy of feeling which we should be wrong in supposing the Mahometans not to be capable of, they did not then omit, they have not since omitted, to remark, that the assassin and his three accomplices were not born on the banks of the Nile.

The army, to mitigate its grief, desired that the funeral of Kléber should be celebrated with great pomp. It wished, also, that on that solemn day, some person should recount the long series of brilliant actions which will transmit the name of the illustrious general to the remotest posterity. By unanimous consent this honourable and perilous mission was confided to Fourier.

There are very few individuals, Gentlemen, who have not seen the brilliant dreams of their youth wrecked one after the other

against the sad realities of mature age. Fourier was one of those few exceptions.

In effect, transport yourselves mentally back to the year 1789, and consider what would be the future prospects of the humble convert of St. Benoît-sur-Loire. No doubt a small share of literary glory; the favour of being heard occasionally in the churches of the metropolis; the satisfaction of being appointed to eulogise such or such a public personage. Well! nine years have hardly passed and you find him at the head of the Institute of Egypt, and he is the oracle, the idol of a society which counted among its members Bonaparte, Berthollet, Monge, Malus, Geoffroy St. Hilaire, Conté, &c.; and the generals rely upon him for overcoming apparently insurmountable difficulties, and the army of the East, itself so rich in adornments of all kinds, would desire no other interpreter when it is necessary to recount the lofty deeds of the hero which it had just lost.

It was upon the breach of a bastion which our troops had recently taken by assault, in sight of the most majestic of rivers, of the magnificent valley which it fertilises, of the frightful desert of Lybia, of the colossal pyramids of Gizeh; it was in presence of twenty populations of different origins which Cairo unites together in its vast basin; in presence of the most valiant soldiers that had ever set foot on a land, wherein, however, the names of Alexander and of Cæsar still resound; it was in the midst of everything which could move the heart, excite the ideas, or exalt the imagination, that Fourier unfolded the noble life of Kléber. The orator was listened to with religious silence; but soon, addressing himself with a gesture of his hand to the soldiers ranged in battle array before him, he exclaims: "Ah! how many of you would have aspired to the honour of throwing yourselves between Kléber and his assassin! I call you to witness, intrepid cavalry, who rushed to save him upon the heights of Koraïm, and dispelled in an instant the multitude of enemies who had surrounded him!" At these words an electric tremor thrills throughout the whole army, the colours droop, the ranks close, the arms come into collision, a deep sigh escapes from some ten thousand breasts torn by the sabre and the bullet, and the voice of the orator is drowned amid sobs.

A few months after, upon the same bastion, before the same soldiers, Fourier celebrated with no less eloquence the exploits, the virtues of the general whom the people conquered in Africa saluted with the name so flattering of *Just Sultan;* and who sacrificed his life at Marengo to secure the triumph of the French arms.

Fourier quitted Egypt only with the last wreck of the army, in virtue of the capitulation signed by Menou. On his return to

France, the object of his most constant solicitude was to illustrate the memorable expedition of which he had been one of the most active and most useful members. The idea of collecting together the varied labours of all his colleagues incontestably belongs to him. I find the proof of this in a letter still unpublished, which he wrote to Kléber from Thebes, on the 20th Vendémiaire, in the year VII. No public act, in which mention is made of this great literary monument, is of an earlier date. The Institute of Cairo having adopted the project of a *work upon Egypt* as early as the month of Frimaire, in the year VIII., confided to Fourier the task of uniting together the scattered elements of it, of making them consistent with each other, and drawing up the general introduction.

This introduction was published under the title of *Historical Preface:* Fontanes saw in it the graces of Athens and the wisdom of Egypt united together. What could I add to such an eulogium? I shall say only that there are to be found there, in a few pages, the principal features of the government of the Pharaohs, and the results of the subjection of ancient Egypt by the kings of Persia, the Ptolemies, the successors of Augustus, the emperors of Byzantium, the first Caliphs, the celebrated Saladin, the Mamelukes and the Ottoman princes. The different phases of our adventurous expedition are there characterised with the greatest care. Fourier carries his scruples to so great a length as *to attempt* to prove that it was just: I have said only so far as *to attempt,* for in that case there might have been something to deduct from the second part of the eulogium of Fontanes. If, in 1797, our countryman experienced at Cairo, or at Alexandria, outrages and extortions which the Grand Seignior either would not or could not repress, one may in all rigour admit that France ought to have exacted justice to herself; that she had the right to send a powerful army to bring the Turkish Custom-house officers to reason. But this is far from maintaining that the divan of Constantinople ought to have favoured the French expedition; that our conquest was about to restore to him, *in some sort,* Egypt and Syria; that the capture of Alexandria and the battle of the *Pyramids would enhance the lustre of the Ottoman name!* However, the public hastened to acquit Fourier of what appears hazarded in this small part of his beautiful work. The origin of it has been sought for in political exigencies. Let us be brief; behind certain sophisms the hand of the original Commander-in-Chief of the army of the East was suspected to be seen!

Napoleon then would appear to have participated by his instructions, by his counsels, or, if we choose, by his imperative orders, in the composition of the essay of Fourier. What was not long ago

ıothing more than a plausible conjecture, has now become an incon-estable fact. Thanks to the courtesy of M. Champollion-Figeac, held in my hands, within the last few days, some parts of the irst *proof sheets* of the historical preface. These proofs were ent to the Emperor, who wished to make himself acquainted with hem at leisure before reading them with Fourier. They are overed with marginal notes, and the additions which they have occasioned amount to almost a third of the original discourse. Upon these pages, as in the definitive work given to the public, one remarks a complete absence of proper names: the only ex-eption is in the case of the three Generals-in-Chief. Thus Fourier had imposed upon himself the reserve which certain vanities have blamed so severely. I shall add that nowhere throughout the precious proof sheets of M. Champollion do we perceive traces of the miserable feelings of jealousy which have been attributed to Napoleon. It is true that upon pointing out with his finger the word illustrious applied to Kléber, the Emperor said to our colleague: "SOME ONE has directed my attention to THIS EPITHET;" but, after a short pause, he added, "it is desirable that you should leave it, for it is just and well de-served." These words, Gentlemen, honoured the monarch still less than they branded with disgrace the *some one* whom I regret not being able to designate in more definite terms,—one of those vile courtiers whose whole life is occupied in spying out the frailties, the evil passions of their masters, in order to make them subservient in conducting themselves to honours and fortune!

FOURIER PREFECT OF L'ISÈRE.

Fourier had no sooner returned to Europe, than he was named (January 2nd, 1802) Prefect of the department of l'Isère. The Ancient Dauphiny was then a prey to ardent political dissensions. The republicans, the partisans of the emigrants, those who had ranged themselves under the banners of the consular government, formed so many distinct castes, between whom all reconciliation appeared impossible. Well, Gentlemen, this impossibility Fourier achieved. His first care was to cause the Hôtel of the Prefecture to be considered as a neutral ground, where each might show him-self without even the appearance of a concession. Curiosity alone at first brought the people there, but the people returned; for in France they seldom desert the saloons wherein are to be found a polished and benevolent host, witty without being ridiculous, and learned without being pedantic. What had been divulged of the opinions of our colleague, respecting the anti-biblican antiquity of the Egyptian monuments, inspired the religious classes especially

with lively apprehensions; they were very adroitly informed that the new prefect counted a *Saint* in his family; that the *blessed* Pierre Fourier, who established the religious sisters of the congregation of Notre-Dame, was his grand uncle, and this circumstance effected a reconciliation which the unalterable respect of the first magistrate of Grenoble for all conscientious opinions cemented every day more and more.

As soon as he was assured of a truce with the political and religious parties, Fourier was enabled to devote himself exclusively to the duties of his office. These duties did not consist with him in heaping up old papers to no advantage. He took personal cognisance of the projects which were submitted to him; he was the indefatigable promoter of all those which narrow-minded persons sought to stifle in their birth; we may include in this last class, the superb road from Grenoble to Turin by Mount Genèvre, which the events of 1814 have so unfortunately interrupted, and especially the drainage of the marshes of Bourgoin.

These marshes, which Louis XIV. had given to Marshal Turenne, were a focus of infection to the thirty-seven communes, the lands of which were partially covered by them. Fourier directed personally the topographic operations which established the possibility of drainage. With these documents in his hand he went from village to village, I might almost say from house to house, to fix the sacrifice which each family ought to impose upon itself for the general interest. By tact and perseverance, taking "the *ear of corn always in the right direction*," thirty-seven municipal councils were induced to contribute to a common fund, without which the projected operation would not even have been commenced. Success crowned this rare perseverance. Rich harvests, fat pastures, numerous flocks, a robust and happy population now covered an immense territory, where formerly the traveller dared not remain more than a few hours.

One of the predecessors of Fourier, in the situation of perpetual secretary of the Academy of Sciences, deemed it his duty, on one occasion, to beg an excuse for having given a detailed account of certain researches of Leibnitz, which had not required great efforts of the intellect: "We ought," says he, "to be very much obliged to a man such as he is, when he condescends, for the public good, to do something which does not partake of genius!" I cannot conceive the ground of such scruples: in the present day, the sciences are regarded from too high a point of view, that we should hesitate in placing in the first rank of the labours with which they are adorned, those which diffuse comfort, health, and happiness amidst the working population.

In presence of a part of the Academy of Inscriptions, in an

apartment wherein the name of hieroglyph has so often resounded, I cannot refrain from alluding to the service which Fourier rendered to science by retaining Champollion. The young professor of history of the Faculty of Letters of Grenoble had just attained the twentieth year of his age. Fate calls him to shoulder the musket. Fourier exempts him by investing him with the title of pupil of the School of Oriental Languages which he had borne at Paris. The Minister of War learns that the pupil formerly gave in his resignation; he denounces the fraud, and dispatches a peremptory order for his departure, which seems even to exclude all idea of remonstrance. Fourier, however, is not discouraged; his intercessions are skilful and of a pressing nature; finally, he draws so animated a portrait of the precocious talent of *his young friend*, that he succeeds in wringing from the government an order of special exemption. It was not easy, Gentlemen, to obtain such success. At the same time, a conscript, a *member of our Academy*, succeeded in obtaining a revocation of his order for departure only by declaring that he would follow on foot in the costume of the Institute, the contingent of the arrondissement of Paris in which he was classed.

MATHEMATICAL THEORY OF HEAT.

The administrative duties of the prefect of l'Isère hardly interrupted the labours of the geometer and the man of letters. It is from Grenoble that the principal writings of Fourier are dated; it was at Grenoble that he composed the *Théorie Mathématique de la Chaleur*, which forms his principal title to the gratitude of the scientific world.

I am far from being unconscious of the difficulty of analysing that admirable work, and yet I shall attempt to point out the successive steps which he has achieved in the advancement of science. You will listen to me, Gentlemen, with indulgence notwithstanding several minute details which I shall have to recount, since I thereby fulfil the mission with which you have honoured me.

The ancients had a taste, let us say rather a passion, for the marvellous, which caused them to forget even the sacred duties of gratitude. Observe them, for example, grouping together the lofty deeds of a great number of heroes, whose names they have not even deigned to preserve, and investing the single personage of Hercules with them. The lapse of ages has not rendered us wiser in this respect. In our own time the public delight in blending fable with history. In every career of life, in the pursuit of science especially, they enjoy a pleasure in creating Herculeses.

According to vulgar opinion, there is no astronomical discovery which is not due to Herschel. The theory of the planetary movements is identified with the name of Laplace; hardly is a passing allusion made to the eminent labours of D'Alembert, of Clairaut, of Euler, of Lagrange. Watt is the sole inventor of the steam-engine. Chaptal has enriched the arts of chemistry with the totality of the fertile and ingenious processes which constitute their prosperity. Even within this apartment has not an eloquent voice lately asserted, that before Fourier the phenomenon of heat was hardly studied; that the celebrated geometer had alone made more observations than all his predecessors put together; that he had with almost a single effort invented a new science.

Although he runs the risk of being less lively, the organ of the Academy of Sciences cannot permit himself such bursts of enthusiasm. He ought to bear in mind, that the object of these solemnities is not merely to celebrate the discoveries of academicians; that they are also designed to encourage modest merit; that an observer forgotten by his contemporaries, is frequently supported in his laborious researches by the thought that he will obtain a benevolent look from posterity. Let us act, so far as it depends upon us, in such a manner that a hope so just, so natural, may not be frustrated. Let us award a just, a brilliant homage to those rare men whom nature has endowed with the precious privilege of arranging a thousand isolated facts, of making seductive theories spring from them; but let us not forget to state, that the scythe of the reaper had cut the stalks before one had thought of uniting them into sheaves!

Heat presents itself in natural phenomena, and in those which are the products of art under two entirely distinct forms, which Fourier has separately considered. I shall adopt the same division, commencing however with radiant heat, the historical analysis which I am about to submit to you.

Nobody doubts that there is a physical distinction which is eminently worthy of being studied between the ball of iron at the ordinary temperature which may be handled at pleasure, and the ball of iron of the same dimensions which the flame of a furnace has very much heated, and which we cannot touch without burning ourselves. This distinction, according to the majority of physical inquirers, arises from a certain quantity of an elastic imponderable fluid, or at least a fluid which has not been weighed, with which the second ball has combined during the process of heating. The fluid which, upon combining with cold bodies renders them hot, has been designated by the name of *heat* or *caloric.*

Bodies unequally heated act upon each other *even at great distances, even through empty space*, for the colder becomes more hot,

and the hotter becomes more cold; for after a certain time they indicate the same degree of the thermometer, whatever may have been the difference of their original temperatures. According to the hypotheses above explained, there is but one way of conceiving this action at a distance: this is to suppose that it operates by the aid of certain effluvia which traverse space by passing from the hot body to the cold body; that is, to admit that a hot body emits in every direction rays of heat, as luminous bodies emit rays of light.

The effluvia, the radiating emanations by the aid of which two distant bodies form a calorific communication with each other, have been very appropriately designated by the name of *radiating caloric.*

Whatever may be said to the contrary, radiating heat had already been the object of important experiments before Fourier undertook his labours. The celebrated academicians of the *Cimento* found, nearly two centuries ago, that this heat is reflected like light; that, as in the case of light, a concave mirror concentrates it at the focus. Upon substituting balls of snow for heated bodies, they even went so far as to prove that frigorific foci may be formed by way of reflexion. Some years afterwards Mariotte, a member of this Academy, discovered that there exist different kinds of radiating heat; that the heat with which rays of light are accompanied traverses all transparent media as easily as light does; while, again, the caloric which emanates from a strongly heated, but opaque substance, while the rays of heat, which are found mingled with the luminous rays of a body moderately incandescent, are almost entirely arrested in their passage through the most transparent plate of glass!

This striking discovery, let us remark in passing, will show, notwithstanding the ridicule of pretended savans, how happily inspired were the workmen in foundries, who looked at the incandescent matter of their furnaces, only through a plate of ordinary glass, thinking by the aid of this artifice to arrest the heat which would have burned their eyes.

In the experimental sciences, the epochs of the most brilliant progress are almost always separated by long intervals of almost absolute repose. Thus, after Mariotte, there elapsed more than a century without history having to record any new property of radiating heat. Then, in close succession, we find in the solar light obscure calorific rays, the existence of which could admit of being established only with the thermometer, and which may be completely separated from luminous rays by the aid of the prism; we discover, by the aid of terrestrial bodies, that the emission of caloric rays, and consequently the cooling of those bodies, is considerably retarded by the polish of the surfaces; that the colour, the nature, and the

thickness of the outer coating of these same surfaces, exercise also a manifest influence upon their emissive power. Experience, finally, rectifying the vague predictions to which the most enlightened minds abandon themselves with so little reserve, shows that the calorific rays which emanate from the plane surface of a heated body have not the same force, the same intensity in all directions; that the *maximum* corresponds to the perpendicular emission, and the *minimum* to the emissions parallel to the surface.

Between these two extreme positions, how does the diminution of the emissive power operate? Leslie first sought the solution of this important question. His observations seem to show that the intensities of the radiating rays are proportional (it is necessary, Gentlemen, that I employ the scientific expression) to the sines of the angles which these rays form with the heated surface. But the quantities upon which the experimenter had to operate were too feeble; the uncertainties of the thermometric estimations compared with the total effect were, on the contrary, too great not to inspire a strong degree of distrust: well, Gentlemen, a problem before which all the processes, all the instruments of modern physics have remained powerless, Fourier has completely solved without the necessity of having recourse to any new experiment. He has traced the law of the emission of caloric sought for, with a perspicuity which one cannot sufficiently admire, in the most ordinary phenomena of temperature, in the phenomena which at first sight appeared to be entirely independent of it.

Such is the privilege of genius; it perceives, it seizes relations where vulgar eyes see only isolated facts.

Nobody doubts, and besides experiment has confirmed the fact, that in all the points of a space terminated by any envelope maintained at a constant temperature, we ought also to experience a constant temperature, and precisely that of the envelope. Now Fourier has established, that if the calorific rays emitted were equally intense in all directions, if the intensity did not vary proportionally to the sine of the angle of emission, the temperature of a body situated in the enclosure would depend on the place which it would occupy there: *that the temperature of boiling water or of melting iron, for example, would exist in certain points of a hollow envelope of glass!* In all the vast domain of the physical sciences, we should be unable to find a more striking application of the celebrated method of the *reductio ad absurdum* of which the ancient mathematicians made use, in order to demonstrate the abstract truths of geometry.

I shall not quit this first part of the labours of Fourier without adding, that he has not contented himself with demonstrating with so much felicity the remarkable law which connects the compa-

ative intensities of the calorific rays, emanating under all angles from heated bodies; he has sought, moreover, the physical cause of his law, and he has found it in a circumstance which his predecessors had entirely neglected. Let us suppose, says he, that bodies emit heat not only from the molecules of their surfaces, but also from the particles in the interior. Let us suppose, moreover, that the heat of these latter particles cannot arrive at the surface by traversing a certain thickness of matter without undergoing some degree of absorption. Fourier has reduced these two hypotheses to calculation, and he has hence deduced mathematically the experimental law of the sines. After having resisted so radical a test, the two hypotheses were found to be completely verified, they have become laws of nature; they point out latent properties of caloric which could only be discerned by the eye of the intellect.

In the second question treated by Fourier, heat presents itself under a new form. There is more difficulty in following its movements; but the conclusions deducible from the theory are also more general and more important.

Heat excited, concentrated into a certain point of a solid body, communicates itself by way of conduction, first to the particles nearest the heated point, then gradually to all the regions of the body. Whence the problem of which the following is the enunciation.

By what routes, and with what velocities, is the propagation of heat effected in bodies of different forms and different natures subjected to certain initial conditions?

Fundamentally, the Academy of Sciences had already proposed this problem as the subject of a prize as early as the year 1736. Then the terms heat and caloric were not in use; it demanded *the study of nature, and the propagation* OF FIRE! The word *fire*, thrown thus into the programme without any other explanation, gave rise to a mistake of the most singular kind. The majority of philosophers imagined that the question was to explain in what way *burning* communicates itself, and increases in a mass of combustible matter. Fifteen competitors presented themselves; *three* were crowned.

This competition was productive of very meagre results. However, a singular combination of circumstances and of proper names will render the recollection of it lasting.

Has not the public a right to be surprised upon reading this Academic declaration: "the question affords no handle to geometry!" In matter of inventions, to attempt to dive into the future, is to prepare for oneself striking mistakes. One of the competitors, the great Euler, took these words in their literal sense;

the reveries, with which his memoir abounds, are not compensated in this instance by any of those brilliant discoveries in analysis, I had almost said of those sublime inspirations, which were so familiar to him. Fortunately Euler appended to his memoir a supplement truly worthy of his genius. Father Lozeran de Fiesc and the Count of Créqui were rewarded with the high honour of seeing their names inscribed beside that of the illustrious geometer, although it would be impossible in the present day to discern in their memoirs any kind of merit, not even that of politeness, for the courtier said rudely to the Academy: "the question, which you have raised, interests only the curiosity of mankind."

Among the competitors less favourably treated, we perceive one of the greatest writers whom France has produced: the author of the *Henriade*. The memoir of Voltaire was, no doubt, far from solving the problem proposed; but it was at least distinguished by elegance, clearness, and precision of language; I shall add, by a severe style of argument; for if the author occasionally arrives at questionable results, it is only when he borrows false data from the chemistry and physics of the epoch, — sciences which had just sprung into existence. Moreover, the anti-Cartesian colour of some of the parts of the memoir of Voltaire was calculated to find little favour in a society, where Cartesianism, with its incomprehensible vortices, was everywhere held in high estimation.

We should have more difficulty in discovering the causes of the failure of a fourth competitor, Madame the Marchioness du Châtelet, for she also entered into the contest instituted by the Academy. The work of Emilia was not only an elegant portrait of all the properties of heat, known then to physical inquirers, there were remarked moreover in it, different projects of experiments, among the rest one which Herschel has since developed, and from which he has derived one of the principal flowers of his brilliant scientific crown.

While such great names were occupied in discussing this question, physical inquirers of a less ambitious stamp laid experimentally the solid basis of a future mathematical theory of heat. Some established, that the same quantity of caloric do not elevate by the same number of degrees equal weights of different substances, and thereby introduced into the science the important notion of *capacity*. Others, by the aid of observations no less certain, proved that heat, applied at the extremity of a bar, is transmitted to the extreme parts with greater or less velocity or intensity, according to the nature of the substance of which the bar is composed: thus they suggested the original idea of *conductibility*. The same epoch, if I were not precluded from entering into too minute details, would present to us interesting experi-

ments. We should find that it is not true that, at all degrees of the thermometer, the loss of heat of a body is proportional to the excess of its temperature above that of the medium in which it is plunged; but I have been desirous of showing you geometry penetrating, timidly at first, into questions of the propagation of heat, and depositing there the first germs of its fertile methods.

It is to Lambert of Mulhouse, that we owe this first step. This ingenious geometer had proposed a very simple problem which any person may comprehend. A slender metallic bar is exposed at one of its extremities to the constant action of a certain focus of heat. The parts nearest the focus are heated first. Gradually the heat communicates itself to the more distant parts, and, after a short time, each point acquires the maximum temperature which it can ever attain. Although the experiment were to last a hundred years, the thermometric state of the bar would not undergo any modification.

As might be reasonably expected, this maximum of heat is so much less considerable as we recede from the focus. Is there any relation between the final temperatures and the distances of the different particles of the bar from the extremity directly heated? Such a relation exists. It is very simple. Lambert investigated it by calculation, and experience confirmed the results of theory.

In addition to the somewhat elementary question of the *longitudinal* propagation of heat, there offered itself the more general but much more difficult problem of the propagation of heat in a body of three dimensions terminated by any surface whatever. This problem demanded the aid of the higher analysis. It was Fourier who first assigned the equations. It is to Fourier, also, that we owe certain theorems, by means of which we may ascend from the differential equations to the integrals, and push the solutions in the majority of cases to the final numerical applications.

The first memoir of Fourier on the theory of heat dates from the year 1807. The Academy, to which it was communicated, being desirous of inducing the author to extend and improve his researches, made the question of the propagation of heat the subject of the great mathematical prize which was to be awarded in the beginning of the year 1812. Fourier did, in effect, compete, and his memoir was crowned. But, alas! as Fontenelle said: "In the country even of demonstrations, there are to be found causes of dissension." Some restrictions mingled with the favourable judgment. The illustrious commissioners of the prize, Laplace, Lagrange, and Legendre, while acknowledging the novelty and importance of the subject, while declaring that the real differential equations of the propagation of heat were finally found, asserted that they perceived difficulties in the way in which the author

arrived at them. They added, that his processes of integration left something to be desired, even on the score of rigour. They did not, however, support their opinion by any arguments.

Fourier never admitted the validity of this decision. Even at the close of his life he gave unmistakeable evidence that he thought it unjust, by causing his memoir to be printed in our volumes without changing a single word. Still, the doubts expressed by the Commissioners of the Academy reverted incessantly to his recollection. From the very beginning they had poisoned the pleasure of his triumph. These first impressions, added to a high susceptibility, explain how Fourier ended by regarding with a certain degree of displeasure the efforts of those geometers who endeavoured to improve his theory. This, Gentlemen, was a very strange aberration of a mind of so elevated an order! Our colleague had almost forgotten that it is not allotted to any person to conduct a scientific question to a definitive termination, and that the important labours of D'Alembert, Clairaut, Euler, Lagrange, and Laplace, while immortalising their authors, have continually added new lustre to the imperishable glory of Newton. Let us act so that this example may not be lost. While the civil law imposes upon the tribunes the obligation to assign the motives of *their judgments*, the academies, which are the tribunes of science, cannot have even a pretext to escape from this obligation. Corporate bodies, as well as individuals, act wisely when they reckon in every instance only upon the authority of reason.

CENTRAL HEAT OF THE TERRESTRIAL GLOBE.

At any time the *Théorie Mathématique de la Chaleur* would have excited a lively interest among men of reflection, since, upon the supposition of its being complete, it threw light upon the most minute processes of the arts. In our own time the numerous points of affinity existing between it and the curious discoveries of the geologists have made it, if I may use the expression, a work for the occasion. To point out the intimate relation which exists between these two kinds of researches would be to present the most important part of the discoveries of Fourier, and to show how happily our colleague, by one of those inspirations reserved for genius, had chosen the subject of his researches.

The parts of the earth's crust, which the geologists call the sedimentary formations, were not formed all at once. The waters of the ocean, on several former occasions, covered regions which are situated in the present day in the centre of the continent. There they deposited, in thin horizontal strata, a series of rocks of different kinds. These rocks, although superposed like the layers of

stones of a wall, must not be confounded together; their dissimilarities are palpable to the least practised eye. It is necessary also to note this capital fact, that each stratum has a well-defined limit; that no process of transition connects it with the stratum which it supports. The ocean, the original source of all these deposits, underwent then formerly enormous changes in its chemical composition to which it is no longer subject.

With some rare exceptions, resulting from local convulsions the effects of which are otherwise manifest, the order of antiquity of the successive strata of rocks which form the exterior crust of the globe ought to be that of their superposition. The deepest have been formed at the most remote epochs. The attentive study of these different envelopes may aid us in ascending the stream of time, even beyond the most remote epochs, and enlightening us with respect to those stupendous revolutions which periodically overwhelmed continents beneath the waters of the ocean, or again restored them to their former condition. Crystalline rocks or granite upon which the sea has effected its original deposits have never exhibited any remains of life. Traces of such are to be found only in the sedimentary strata.

Life appears to have first exhibited itself on the earth in the form of vegetables. The remains of vegetables are all that we meet with in the most ancient strata deposited by the waters; still they belong to plants of the simplest structure,—to ferns, to species of rushes, to lycopodes.

As we ascend into the upper strata, vegetation becomes more and more complex. Finally, near the surface, it resembles the vegetation actually existing on the earth, with this characteristic circumstance, however, which is well deserving attention, that certain vegetables which grow only in southern climates, that the large palm trees for example, are found in their fossil state in all latitudes, and even in the centre of the frozen regions of Siberia.

In the primitive world, these northern regions enjoyed then, in winter, a temperature at least equal to that which is experienced in the present day under the parallels where the great palms commence to appear: at Tobolsk, the inhabitants enjoyed the climate of Alicante or Algiers!

We shall deduce new proofs of this mysterious result from an attentive examination of the size of plants.

There exist, in the present day, willow grass or marshy rushes ferns, and lycopodes, in Europe as well as in the tropical regions but they are not met with in large dimensions, except in warm countries. Thus, to compare together the dimensions of the same plants is, in reality, to compare, in respect to temperature, the regions where they are produced. Well, place beside the fossil

plants of our coal mines, I will not say the analogous plants of Europe, but those which grow in the countries of South America, and which are most celebrated for the richness of their vegetation, and you will find the former to be of incomparably greater dimensions than the latter.

The *fossil flora* of France, England, Germany, and Scandinavia offer, for example, ferns ninety feet high, the stalks being six feet in diameter or eighteen feet in circumference.

The *lycopodes* which, in the present day, whether in cold or temperate climates, are creeping-plants rising hardly to the height of a decimètre above the soil; which, even at the equator, under the most favourable circumstances, do not attain a height of more than *one* mètre, had in Europe, in the primitive world, an altitude of twenty-five mètres.

One must be blind to all reason not to find, in these enormous dimensions, a new proof of the high temperature enjoyed by our country before the last irruptions of the ocean!

The study of *fossil animals* is no less fertile in results. I should digress from my subject if I were to examine here how the organisation of animals is developed upon the earth; what modifications, or more strictly speaking, what complications it has undergone after each cataclysm, or if I even stopped to describe one of those ancient epochs during which the earth, the sea, and the atmosphere had for inhabitants cold-blooded reptiles of enormous dimensions; tortoises with shells three feet in diameter; lizards seventeen mètres long; pterodactyles, veritable flying dragons of such strange forms, that they might be classed on good grounds either among reptiles, among mammiferous animals or among birds. The object, which I have proposed, does not require that I should enter into such details; a single remark will suffice.

Among the bones contained in the strata nearest the present surface of the earth, are those of the hippopotamus, the rhinoceros, and the elephant. These remains of animals of warm countries are to be found in all latitudes. Travellers have discovered specimens of them even at Melville Island, where the temperature descends, in the present day, 50° beneath zero. In Siberia they are found in such abundance as to have become an article of commerce. Finally, upon the rocky shores of the Arctic Ocean, there are to be found not merely fragments of skeletons, but whole elephants still covered with their flesh and skin.

I should deceive myself very much, Gentlemen, if I were to suppose that each of you had not deduced from these remarkable facts a conclusion no less remarkable, to which indeed the fossil flora had already habituated us; namely, that as they have grown

older, the polar regions of the earth have cooled down to a prodigious extent.

In the explanation of so curious a phenomenon, cosmologists have not taken into account the existence of possible variations of the intensity of the solar heat; and yet the stars, those distant suns, have not the constant brightness which the common people attribute to them. Nay, some of them have been observed to diminish in a sufficiently short space of time to the hundredth part of their original brightness; and several have even totally disappeared. They have preferred to attribute everything to an internal or primitive heat with which the earth was at some former epoch impregnated, and which is gradually being dissipated in space.

Upon this hypothesis the inhabitants of the polar regions, although deprived of the sight of the sun for whole months together, must have evidently enjoyed, at very ancient epochs, a temperature equal to that of the tropical regions, wherein exist elephants in the present day.

It is not, however, as an explanation of the existence of elephants in Siberia, that the idea of the intrinsic heat of the globe has entered for the first time into science. Some savans had adopted it before the discovery of those fossil animals. Thus, Descartes was of opinion that originally (I cite his own words), *the earth did not differ from the sun in any other respect than in being smaller*. Upon this hypothesis, then, it ought to be considered as an extinct sun.

Leibnitz conferred upon this hypothesis the honour of appropriating it to himself. He attempted to deduce from it the mode of formation of the different solid envelopes of which the earth consists. Buffon, also, imparted to it the weight of his eloquent authority. According to that great naturalist, the planets of our system are merely portions of the sun, which the shock of a comet had detached from it some tens of thousands of years ago.

In support of this igneous origin of the earth, Mairan and Buffon cited already the high temperature of deep mines, and, among others, those of the mines of Giromagny. It appears evident that if the earth was formerly incandescent, we should not fail to meet in the interior strata, that is to say, in those which ought to have cooled last, traces of their primitive temperature. The observer who, upon penetrating into the interior of the earth, did not find an increasing heat, might then consider himself amply authorised to reject the hypothetical conceptions of Descartes, of Mairan, of Leibnitz, and of Buffon. But has the converse proposition the same certainty? Would not the torrents of heat, which the sun has continued incessantly to launch for so many ages, have diffused themselves into the mass of the earth, so as to produce there

a temperature increasing with the depth? This is a question of high importance. Certain easily satisfied minds conscientiously supposed that they had solved it, when they stated that the idea of a constant temperature was by far the *most natural;* but woe to the sciences if they thus included vague considerations, which escape all criticism, among the motives for admitting and rejecting facts and theories! Fontenelle, Gentlemen, would have traced their horoscope in these words, so well adapted for humbling our pride, and the truth of which the history of discoveries reveals in a thousand places: "When a thing may be in two different ways, it is almost always that which appears at first the least natural."

Whatever importance these reflections may possess, I hasten to add that, instead of the arguments of his predecessors, which have no real value, Fourier has substituted proofs, demonstrations; and we know what meaning such terms convey to the Academy of Sciences.

In all places of the earth, as soon as we descend to a certain depth, the thermometer no longer experiences either diurnal or annual variation. It marks the same degree, and the same fraction of a degree, from day to day, and from year to year. Such is the fact: what says theory?

Let us suppose, for a moment, that the earth has constantly received all its heat from the sun. Descend into its mass to a sufficient depth, and you will find, with Fourier, by the aid of calculation, a constant temperature for each day of the year. You will recognise further, that this solar temperature of the inferior strata varies from one climate to another; that in each country, finally, it ought to be always the same, so long as we do not descend to depths which are too great relatively to the earth's radius.

Well, the phenomena of nature stand in manifest contradiction to this result. The observations made in a multitude of mines, observations of the temperature of hot springs coming from different depths, have all given an increase of one degree of the centigrade for every twenty or thirty mètres of depth. Thus, there was some inaccuracy in the hypothesis which we were discussing upon the footsteps of our colleague. It is not true that the temperature of the terrestrial strata may be attributed solely to the action of the solar rays.

This being established, the increase of heat which is observed in all climates when we penetrate into the interior of the globe, is the manifest indication of an intrinsic heat. The earth, as Descartes and Leibnitz maintained it to be, but without being able to support their assertions by any demonstrative reasoning,—thanks to a combination of the observations of physical inquirers with the analytical calculations of Fourier,—is *an encrusted sun,* the high

temperature of which may be boldly invoked every time that th explanation of ancient geological phenomena will require it.

After having established that there is in our earth an inheren heat,—a heat the source of which is not the sun, and which, if w may judge of it by the rapid increase which observation indicates ought to be already sufficiently intense at the depth of only seve or eight leagues to hold in fusion all known substances,—ther arises the question, what is its precise value at the surface of th earth; what weight are we to attach to it in the determination o terrestrial temperatures; what part does it play in the phenomen of life?

According to Mairan, Buffon, and Bailly, this part is immense For France, they estimate the heat which escapes from the interio of the earth, at twenty-nine times in summer, and four hundre times in winter, the heat which comes to us from the sun. Thus contrary to general opinion, the heat of the body which illuminate us would form only a very small part of that whose propitious in fluence we feel.

This idea was developed with ability and great eloquence in th *Memoirs of the Academy*, in the *Epoques sur la Nature* of Buffon in the letters from Bailly to Voltaire *upon the Origin of the Science and upon the Atlantide.* But the ingenious romance to which i has served as a base, has vanished like a shadow before the torc of mathematical science.

Fourier having discovered that the excess of the aggregate tem perature of the earth's surface above that which would result fror the sole action of the solar rays, has a determinate relation to th increase of temperature at different depths, succeeded in deducin from the experimental value of this increase a numerical determina tion of the excess in question. This excess is the thermometri effect which the solar heat produces at the surface; now, instead o the large numbers adopted by Mairan, Bailly, and Buffon, what ha our colleague found? *A thirtieth* of a degree, not more.

The surface of the earth, which originally was perhaps incan descent, has cooled then in the course of ages, so as hardly to pre serve any sensible trace of its primitive heat. However, at grea depths, the original heat is still enormous. Time will alter sensibl the internal temperature; but at the surface (and the phenomen of the surface can alone modify or compromise the existence o living beings), all the changes are almost accomplished. Th frightful freezing of the earth, the epoch of which Buffon fixe at the instant when the central heat would be totally dissipate is then a pure dream. At the surface, the earth is no longer im pregnated except by the solar heat. So long as the sun sha continue to preserve the same brightness, mankind will find, fror

pole to pole, under each latitude, the climates which have permitted them to live and to establish their residence. These, Gentlemen, are great, magnificent results. While recording them in the annals of science, historians will not neglect to draw attention to this singular peculiarity: that the geometer, to whom we owe the first certain demonstration of the existence of a heat independent of a solar influence in the interior of the earth, has annihilated the immense part which this primitive heat was made to play in the explanation of the phenomena of terrestrial temperature.

Besides divesting the theory of climates of an error which occupied a prominent place in science, supported as it was by the imposing authority of Mairan, of Bailly, and of Buffon, Fourier is entitled to the merit of a still more striking achievement: he has introduced into this theory a consideration which hitherto had been totally neglected; he has pointed out the influence exercised by the *temperature of the celestial regions,* amid which the earth describes its immense orb around the sun.

When we perceive, even under the equator, certain mountains covered with eternal snow, upon observing the rapid diminution of temperature which the strata of the atmosphere undergo during ascents in balloons, meteorologists have supposed, that in the regions wherein the extreme rarity of the air will always exclude the presence of mankind, and that especially beyond the limits of the atmosphere, there ought to prevail a prodigious intensity of cold. It was not merely by hundreds, it was by thousands of degrees, that they had arbitrarily measured it. But, as usual, the imagination (*cette folie de la maison*) had exceeded all reasonable limits. The hundreds, the tens of thousands of degrees, have dwindled down, after the rigorous researches of Fourier, to fifty or sixty degrees only. Fifty to sixty degrees *beneath zero,* such is the temperature which the radiation of heat from the stars has established in the regions furrowed indefinitely by the planets of our system.

You recollect, Gentlemen, with what delight Fourier used to converse upon this subject. You know well that he thought himself sure of having assigned the temperature of space within eight or ten degrees. By what fatality has it happened that the memoir, wherein no doubt our colleague had recorded all the elements of that important determination, is not to be found? May that irreparable loss prove at least to so many observers, that instead of pursuing obstinately an ideal perfection, which it is not allotted to man to attain, they will act wisely in placing the public, as soon as possible, in the confidence of their labours.

I should have yet a long course to pursue, if, after having pointed out some of those problems of which the condition of science enabled our learned colleague to give numerical solutions, I were to

analyse all those which, still enveloped in general formulæ, awai merely the data of experience to assume a place among the mos curious acquisitions of modern physics. Time, which is not at m disposal, precludes me from dwelling upon such developments. should be guilty, however, of an unpardonable omission, if I di not state that, among the formulæ of Fourier, there is one whicl serves to assign the value of the secular cooling of the earth, an in which there is involved the number of centuries which hav elapsed since the origin of this cooling. The question of the antiquity of the earth, including even the period of incandescence which has been so keenly discussed, is thus reduced to a thermometric determination. Unfortunately this point of theory is subjec to serious difficulties. Besides, the thermometric determination in consequence of its excessive smallness, must be reserved fo future ages.

RETURN OF NAPOLEON FROM ELBA.—FOURIER PREFECT OF THE RHONE —HIS NOMINATION TO THE OFFICE OF DIRECTOR OF THE BOARD OI STATISTICS OF THE SEINE.

I have just exhibited to you the scientific fruits of the leisur hours of the Prefect of l'Isère. Fourier still occupied this situatio when Napoleon arrived at Cannes. His conduct during this grav conjuncture has been the object of a hundred false rumours. I shall then discharge a duty by establishing the facts in all thei truth, according to what I have heard from our colleague's ow mouth.

Upon the news of the Emperor having disembarked, the principal authorities of Grenoble assembled at the residence of th Prefect. There each individual explained ably, but especially said Fourier, with much detail, the difficulties which he perceived As regards the means of vanquishing them, the authorities seemed to be much less inventive. Confidence in administrative eloquence was not yet worn out at that epoch; it was resolved accordingly to have recourse to proclamations. The commanding officer and the Prefect presented each a project. The assembly was discussing minutely the terms of them, when an officer of the gendarmes, an old soldier of the Imperial armies, exclaimed rudely, "Gentlemen, be quick, otherwise all deliberation will become useless. Believe me, I speak from experience; Napoleon always follows very closely the couriers who announce his arrival." Napoleon was in fact close at hand. After a short moment of hesitation, two companies of sappers which had been dispatched to cut down a bridge, joined their former commander. A battalion of infantry soon followed their example. Finally, upon the very glacis of the fortress, in presence of the numerous population which crowned

the ramparts, the fifth regiment of the line to a man assumed the tricolour cockade, substituted for the white flag the eagle, — witness of twenty battles, — which it had preserved, and departed with shouts of *Vive l'Empereur!* After such a commencement, to attempt to hold the country would have been an act of folly. General Marchand caused accordingly the gates of the city to be shut. He still hoped, notwithstanding the evidently hostile disposition of the inhabitants, to sustain a siege with the sole assistance of the third regiment of engineers, the fourth regiment of artillery, and some weak detachments of infantry which had not abandoned him.

From that moment, the civil authority had disappeared. Fourier thought then that he might quit Grenoble, and repair to Lyons, where the princes had assembled together. At the second restoration, this departure was imputed to him as a crime. He was very near being brought before a court of assizes, or even a provost's court. Certain personages pretended that the presence of the Prefect of the chief place of l'Isère might have conjured the storm; that the resistance might have been more animated, better arranged. People forgot that nowhere, and at Grenoble even less than anywhere else, was it possible to organise even a pretext of resistance. Let us see then, finally, how this martial city, — the fall of which Fourier might have prevented by his mere presence, — let us see how it was taken. It is eight o'clock in the evening. The inhabitants and the soldiers garrison the ramparts. Napoleon precedes his little troop by some steps; he advances even to the gate; he knocks (be not alarmed, Gentlemen, it is not a battle which I am about to describe,) *he knocks with his snuff-box!* "Who is there?" cried the officer of the guard. "It is the Emperor! Open!" — "Sire, my duty forbids me." — "Open — I tell you; I have no time to lose." — "But, sire, even though I should open to you, I could not. The keys are in the possession of General Marchand." — "Go, then, and fetch them." — "I am certain that he will refuse them to me." — "If the General refuse them, *tell him that I will dismiss him.*"

These words petrified the soldiers. During the previous two days, hundreds of proclamations designated Bonaparte as a wild beast which it was necessary to seize without scruple; they ordered everybody to run away from him, and yet this man threatened the general with deprivation of his command! The single word *dismissal*, effaced the faint line of demarcation which separated for an instant the old soldiers from the young recruits; one word established the whole garrison in the interest of the emperor.

The circumstances of the capture of Grenoble were not yet known when Fourier arrived at Lyons. He brought thither the news of the rapid advance of Napoleon; that of the revolt of two

companies of sappers, of a regiment of infantry, and of the regiment commanded by Labédoyère. Moreover, he was a witness of the lively sympathy which the country people along the whole route displayed in favour of the proscribed exile of Elba.

The Count d'Artois gave a very cold reception to the Prefect and his communications. He declared that the arrival of Napoleon at Grenoble was impossible; that no alarm need be apprehended respecting the disposition of the country people. "As regards the facts," said he to Fourier, "which would seem to have occurred in your presence at the very gates of the city, with respect to the tricoloured cockades substituted for the cockade of Henry IV., with respect to the eagles which you say have replaced the white flag, I do not suspect your good faith, but the uneasy state of your mind must have dazzled your eyes. Prefect, return then without delay to Grenoble; you will answer for the city with your head."

You see, Gentlemen, after having so long proclaimed the necessity of telling the truth to princes, moralists will act wisely by inviting princes to be good enough to listen to its language.

Fourier obeyed the order which had just been given him. The wheels of his carriage had made only a few revolutions in the direction of Grenoble, when he was arrested by hussars, and conducted to the head-quarters at Bourgoin. The Emperor, who was engaged in examining a large chart with a pair of compasses, said, upon seeing him enter: "Well, Prefect, you also have declared war against me?"—"Sire, my oath of allegiance made it my duty to do so!"—"A duty you say? and do you not see that in Dauphiny nobody is of the same mind? Do not imagine, however, that your plan of the campaign will frighten me much. It only grieved me to see among my enemies an *Egyptian*, a man who had eaten along with me the bread of the bivouac, an old friend!"

It is painful to add that to those kind words succeeded these also: "How, moreover, could you have forgotten, Monsieur Fourier, that I have made you what you are?"

You will regret with me, Gentlemen, that a timidity, which circumstances would otherwise easily explain, should have prevented our colleague from at once emphatically protesting against this confusion, which the powerful of the earth are constantly endeavouring to establish between the perishable bounties of which they are the dispensers, and the noble fruits of thought. Fourier was Prefect and Baron by the favour of the Emperor; he was one of the glories of France by his own genius!

On the 9th of March, Napoleon, in a moment of anger, ordered Fourier, by a mandate, dated from Grenoble, *to quit the territory*

of the seventh military division within five days, under pain of being arrested and treated as an enemy of the country! On the following day, our colleague departed from the Conference of Bourgoin, with the appointment of Prefect of the Rhone and the title of *Count*, for the Emperor after his return from Elba was again at his old practices.

These unexpected proofs of favour and confidence afforded little pleasure to our colleague, but he dared not refuse them, although he perceived very distinctly the immense gravity of the events in which he was led by the vicissitude of fortune to play a part.

"What do you think of my enterprise?" said the Emperor to him on the day of his departure from Lyons. "Sire," replied Fourier, "I am of opinion that you will fail. Let but a fanatic meet you on your way, and all is at an end."—"Bah!" exclaimed Napoleon, "the Bourbons have nobody on their side, not even a fanatic. In connexion with this circumstance, you have read in the journals that they have excluded me from the protection of the law. I shall be more indulgent on my part; I shall content myself with excluding them from the Tuileries."

Fourier held the appointment of Prefect of the Rhone only till the 1st of May. It has been alleged that he was recalled, because he refused to be accessory to the deeds of terrorism which the minister of the hundred days enjoined him to execute. The Academy will always be pleased when I collect together, and place on record, actions which, while honouring its members, throw new lustre around the entire body. I even feel that, in such a case, I may be disposed to be somewhat credulous. On the present occasion, it was imperatively necessary to institute a most rigorous examination. If Fourier honoured himself by refusing to obey certain orders, what are we to think of the minister of the interior from whom those orders emanated? Now this minister, it must not be forgotten, was also an academician, illustrious by his military services, distinguished by his mathematical works, esteemed and cherished by all his colleagues. Well! I declare, Gentlemen, with a satisfaction which you will all share, that a most scrupulous investigation of all the acts of the hundred days has not disclosed a trace of anything which might detract from the feelings of admiration with which the memory of Carnot is associated in your minds.

Upon quitting the Prefecture of the Rhone, Fourier repaired to Paris. The Emperor, who was then upon the eve of setting out to join the army, perceiving him amid the crowd at the Tuileries, accosted him in a friendly manner, informed him that Carnot would explain to him why his displacement at Lyons had become indispensable, and promised to attend to his interest as soon as

military affairs would allow him some leisure time. The second restoration found Fourier in the capital without employment, and justly anxious with respect to the future. He, who, during a period of fifteen years, administered the affairs of a great department; who directed works of such an expensive nature; who, in the affair of the marshes of Bourgoin, had to contract engagements for so many millions, with private individuals, with the communes and with public companies, had not *twenty thousand francs* in his possession. This honourable poverty, as well as the recollection of glorious and important services, was little calculated to make an impression upon ministers influenced by political passion, and subject to the capricious interference of foreigners. A demand for a pension was accordingly repelled with rudeness. Be reassured, however, France will not have to blush for having left in poverty one of her principal ornaments. The Prefect of Paris,—I have committed a mistake, Gentlemen, a proper name will not be out of place here,—M. Chabrol, learns that his old professor at the Polytechnic School, that the Perpetual Secretary of the Institute of Egypt, that the author of the *Théorie Analytique de la Chaleur*, was reduced, in order to obtain the means of living, to give private lessons at the residences of his pupils. The idea of this revolts him. He accordingly shows himself deaf to the clamours of party, and Fourier receives from him the superior direction of the *Bureau de la Statistique* of the Seine, with a salary of 6000 francs. It has appeared to me, Gentlemen, that I ought not to suppress these details. Science may show herself grateful towards all those who give her support and protection, when there is some danger in doing so, without fearing that the burden should ever become too heavy.

Fourier responded worthily to the confidence reposed in him by M. de Chabrol. The memoirs with which he enriched the interesting volumes published by the Prefecture of the Seine, will serve henceforth as a guide to all those who have the good sense to see in statistics, something else than an indigestible mass of figures and tables.

ENTRANCE OF FOURIER INTO THE ACADEMY OF SCIENCES.—HIS ELECTION TO THE OFFICE OF PERPETUAL SECRETARY.—HIS ADMISSION TO THE FRENCH ACADEMY.

The Academy of Sciences seized the first occasion which offered itself to attach Fourier to its interests. On the 27th of May, 1816, he was nominated a free academician. This election was not confirmed. The solicitations and influence of the Dauphin whom circumstances detained at Paris, had almost disarmed the authorities, when a courtier exclaimed that an amnesty was to

be granted to *the civil Labédoyère!* * This word,—for during many ages past the poor human race has been governed by words,—decided the fate of our colleague. Thanks to political intrigue, the ministers of Louis XVIII. decided that one of the most learned men of France should not belong to the Academy; that a citizen, who enjoyed the friendship of all the most distinguished persons in the metropolis, should be publicly stricken with disapprobation!

In our country, the reign of absurdity does not last long. Accordingly in 1817, when the Academy, without being discouraged by the ill success of its first attempt, unanimously nominated Fourier to the place which had just been vacant in the section of physics, the royal confirmation was accorded without difficulty. I ought to add that soon afterwards, the ruling authorities whose repugnances were entirely dissipated, frankly and unreservedly applauded the happy choice which you made of the learned geometer to replace Delambre as perpetual secretary. They even went so far as to offer him the Directorship of the Fine Arts; but our colleague had the good sense to refuse the appointment.

Upon the death of Lémontey, the French Academy, where Laplace and Cuvier already represented the sciences, called also Fourier into its bosom. The literary titles of the most eloquent of the writers connected with the work on Egypt were incontestable; they even were not contested, and still this nomination excited violent discussions in the journals, which profoundly grieved our colleague. And yet, after all, was it not a fit subject for discussion, whether these double nominations are of any real utility? Might it not be maintained, without incurring the reproach of paradox, that it extinguishes in youth an emulation which we are bound by every consideration to encourage? Besides, with double, triple, and quadruple academicians, what would eventually become of the justly boasted unity of the Institute? Without insisting further on these remarks, the justness of which you will admit if I mistake not, I hasten to repeat that the academic titles of Fourier did not form even the subject of a doubt. The applause which was lavished upon the eloquent éloges of Delambre, of Bréguet, of Charles, and of Herschel, would sufficiently evince that, if their author had not been already one of the most distinguished members of the Academy of Sciences, the public would have invited him to assume a place among the judges of French literature.

CHARACTER OF FOURIER.—HIS DEATH.

Restored at length, after so many vicissitudes, to his favourite

* In allusion to the *military* traitor Colonel Labédoyère, who was condemned to death for espousing the cause of Napoleon.— *Translator.*

pursuits, Fourier passed the last years of his life in retirement and in the discharge of academic duties. *To converse* had become the half of his existence. Those who have been disposed to consider this the subject of just reproach, have no doubt forgotten that constant reflection is no less imperiously forbidden to man than the abuse of physical powers. Repose, in everything, recruits our frail machine; but, Gentlemen, he who desires repose may not obtain it. Interrogate your own recollections and say, if, when you are pursuing a new truth, a walk, the intercourse of society, or even sleep, have the privilege of distracting you from the object of your thoughts? The extremely shattered state of Fourier's health enjoined the most careful attention. After many attempts, he found only one means of escaping from the contentions of mind which exhausted him: this consisted in speaking aloud upon the events of his life; upon his scientific labours, which were either in course of being planned, or which were already terminated; upon the acts of injustice of which he had reason to complain. Every person must have remarked, how insignificant was the state which our gifted colleague assigned to those who were in the habit of conversing with him; we are now acquainted with the cause of this.

Fourier had preserved, in old age, the grace, the urbanity, the varied knowledge which, a quarter of a century previously, had imparted so great a charm to his lectures at the Polytechnic School. There was a pleasure in hearing him relate the anecdote which the listener already knew by heart, even the events in which the individual had taken a direct part. I happened to be a witness of the kind of *fascination* which he exercised upon his audience, in connection with an incident which deserves to be known, for it will prove that the word which I have just employed is not in anywise exaggerated.

We found ourselves seated at the same table. The guest from whom I separated him was an old officer. Our colleague was informed of this, and the question, "Have you been in Egypt?" served as the commencement of a conversation between them. The reply was in the affirmative. Fourier hastened to add: "As regards myself, I remained in that magnificent country until the period of its complete evacuation. Although foreign to the profession of arms, I have, in the midst of our soldiers, fired against the insurgents of Cairo; I have had the honour of hearing the cannon of Heliopolis." Hence to give an account of the battle was but a step. This step was soon made, and we were presented with four battalions drawn up in squares in the plain of Quoubbèh, and manœuvring, with admirable precision, conformably to the orders of the illustrious geometer. My neighbour, with attentive ear, with immovable eyes, and with outstretched neck, listened to this recital with the liveliest interest. He did not lose a single syllable of

it: one would have sworn that he had for the first time heard of those memorable events. Gentlemen, it is so delightful a task to please! After having remarked the effect which he produced, Fourier reverted, with still greater detail, to the principal fight of those great days: to the capture of the fortified village of Mattaryeh, to the passage of two feeble columns of French grenadiers across ditches heaped up with the dead and wounded of the Ottoman army. "Generals, ancient and modern, have sometimes spoken of similar deeds of prowess," exclaimed our colleague, "but it was in the hyperbolic style of the bulletin: here the fact is materially true,—it is true like geometry. I feel conscious, however," added he, "that in order to induce your belief in it, all my assurances will not be more than sufficient."

"Do not be anxious upon this point," replied the officer, who at that moment seemed to awaken from a long dream. "In case of necessity, I might guarantee the accuracy of your statement. It was I who, at the head of the grenadiers of the 13th and 85th semi-brigades, forced the entrenchments of Mattaryeh, by passing over the dead bodies of the Janissaries!"

My neighbour was General Tarayre: you may imagine much better than I can express, the effect of the few words which had just escaped from him. Fourier made a thousand excuses, while I reflected upon the seductive influence, upon the power of language, which for more than half an hour had robbed the celebrated general even of the recollection of the part which he had played in the battle of giants he was listening to.

The more our secretary had occasion to converse, the greater repugnance he experienced to verbal discussions. Fourier cut short every debate as soon as there presented itself a somewhat marked difference of opinion, only to resume afterwards the same subject upon the modest pretext of making a small step in advance each time. Some one asked Fontaine, a celebrated geometer of this Academy, how he occupied his thoughts in society, wherein he maintained an almost absolute silence: "I observe," he replied, "the vanity of mankind, to wound it as occasion offers." If, like his predecessor, Fourier also studied the baser passions which contend for honours, riches, and power, it was not in order to engage in hostilities with them: resolved never to compromise matters with them, he yet so calculated his movements beforehand, as not to find himself in their way. We perceive a wide difference between this disposition and the ardent impetuous character of the young orator of the popular society of Auxerre. But what purpose would philosophy serve, if it did not teach us to conquer our passions? It is not that occasionally the natural disposition of Fourier did not display itself in full relief. "It is strange," said

one day a certain very influential personage of the court of Charles X., whom Fourier's servant would not allow to pass beyond the antechamber of our colleague,—"it is truly strange that your master should be more difficult of access than a minister!" Fourier heard the conversation, leaped out of his bed to which he was confined by indisposition, opened the door of the chamber, and exclaimed, face to face with the courtier: "Joseph, tell Monsieur, that if I was minister, I should receive everybody, because it would be my duty to do so; but, being a private individual, I receive whomsoever I please, and at what hour soever I please!" Disconcerted by the liveliness of the retort, the great seignior did not utter one word in reply. We must even believe that from that moment he resolved not to visit any but ministers, for the plain man of science heard nothing more of him.

Fourier was endowed with a constitution which held forth a promise of long life; but what can natural advantages avail against the anti-hygienic habits which men arbitrarily acquire! In order to guard against slight attacks of rheumatism, our colleague was in the habit of clothing himself, even in the hottest season of the year, after a fashion which is not practised even by travellers condemned to spend the winter amid the snows of the polar regions. "One would suppose me to be corpulent," he used to say occasionally with a smile; "be assured, however, that there is much to deduct from this opinion. If, after the example of the Egyptian mummies, I was subjected to the operation of disembowelment,—from which heaven preserve me,—the residue would be found to be a very slender body." I might add, selecting also my comparison from the banks of the Nile, that in the apartments of Fourier, which were always of small extent, and intensely heated even in summer, the currents of air to which one was exposed, resembled sometimes the terrible simoon, that burning wind of the desert, which the caravans dread as much as the plague.

The prescriptions of medicine which, in the mouth of M. Larrey, were blended with the anxieties of a long and constant friendship, failed to induce a modification of this mortal régime. Fourier had already experienced, in Egypt and Grenoble, some attacks of aneurism of the heart. At Paris, it was impossible to be mistaken with respect to the primary cause of the frequent suffocations which he experienced. A fall, however, which he sustained on the 4th of May, 1830, while descending a flight of stairs, aggravated the malady to an extent beyond what could have been ever feared. Our colleague, notwithstanding pressing solicitations, persisted in refusing to combat the most threatening symptoms, except by the aid of patience and a high temperature. On the 16th of May, 1830, about four o'clock in the evening, Fourier experienced in

his study a violent crisis the serious nature of which he was far from being sensible of; for, having thrown himself completely dressed upon his bed, he requested M. Petit, a young doctor of his acquaintance who carefully attended him, not to go far away, in order, said he, that we may presently converse together. But to these words succeeded soon the cries, "Quick, quick! some vinegar! I am fainting!" and one of the men of science who has shed the brightest lustre upon the Academy had ceased to live.

Gentlemen, this cruel event is too recent, that I should recall here the grief which the Institute experienced upon losing one of its most important members; and those obsequies, on the occasion of which so many persons, usually divided by interests and opinions, united together, in one common feeling of admiration and regret, around the mortal remains of Fourier; and the Polytechnic School swelling in a mass the cortége, in order to render homage to one of its earliest, of its most celebrated professors; and the words which, on the brink of the tomb, depicted so eloquently the profound mathematician, the elegant writer, the upright administrator, the good citizen, the devoted friend. We shall merely state that Fourier belonged to all the great learned societies of the world, that they united with the most touching unanimity in the mourning of the Academy, in the mourning of all France: a striking testimony that the republic of letters is no longer, in the present day, merely a vain name! What, then, was wanting to the memory of our colleague? A more able successor than I have been to exhibit in full relief the different phases of a life so varied, so laborious, so gloriously interlaced with the greatest events of the most memorable epochs of our history. Fortunately, the scientific discoveries of the illustrious secretary had nothing to dread from the incompetency of the panegyrist. My object will have been completely attained if, notwithstanding the imperfection of my sketches, each of you will have learned that the progress of general physics, of terrestrial physics, and of geology, will daily multiply the fertile applications of the *Théorie Analytique de la Chaleur*, and that this work will transmit the name of Fourier down to the remotest posterity.

C A R N O T.

BIOGRAPHY READ AT A PUBLIC ASSEMBLY OF THE ACADEMY OF SCIENCES, ON THE 21ST OF AUGUST, 1837.

CHILDHOOD OF CARNOT. — HIS EDUCATION.

LAZARE-NICOLAS-MARGUERITE CARNOT was born at Nolay (Côte-d'Or), in the ancient duchy of Burgundy that had already been the cradle of three of the greatest ornaments of which the Academies could boast: Bossuet, Vauban, and Buffon. His father was an advocate, and exercised that noble profession with a great deal of talent (which is not uncommon) and with very great disinterestedness (which is said to be not so common). The advocate, Claude-Abraham Carnot, had eighteen children; so that, according to the old adage, which promises prosperity to a numerous family, he might expect a happy future for each of his children. Indeed, at one period, he might have counted, in this numerous family, two lieutenant-generals of the French armies; a councillor at the court of repeal; an attorney-general of the *cour royale;* the directress of the hospice de Nolay; a municipal magistrate much esteemed whilst he was administering the affairs of his corporation, and still more esteemed, if possible, when, after twenty-three years' exercise of his functions, he submitted to be brutally deposed sooner than fail in his duty. I must mention that, like an affectionate and provident father, the advocate of Nolay had not trusted unreservedly to the virtue of the proverb, but always presided personally over the early education of his sons. Lazare Carnot, the subject of this biography, only left his father's house to go, as it was then called, through his Rhetoric and Philosophy.

The childhood of those privileged men who, under various claims, have acted a brilliant part on the stage of the world, has always attracted the attention of every biographer. The "know thyself" of an ancient philosopher would be but poorly interpreted

if only looked on as a maxim of prudence; the maxim is susceptible of a juster and wider interpretation: it presents to us, I think the whole human race, as a body, for the most important species of study that we can undertake. Therefore, Gentlemen, let us carefully examine how those extraordinary minds are indicated, are born, and grow, which, on their complete development, are destined to open out for themselves unknown paths. These characteristics should be collected with all the more interest, because they become daily more rare. In our modern schools, modelled on exactly the same pattern from north to south and from east to west; subjected to the same regulations and to a uniform discipline; where children enter moreover at the age of nine or ten years, and do not leave until they are eighteen or twenty, individual character is effaced, or disappears, or is covered with the mask of conventionality. The agriculturist would never go into a hot-house to learn the character, the form, or the appearance of those admirable plants which are the ornament of our ancient forests. Neither is it in our regiments that one might hope to trace out the true types of the peasants of Brittany, Normandy, Lorraine, or Franche-Comté. Our "school-regiments" (if I may be allowed the expression) would lead moralists quite as much astray. There, a sort of mean is established, about which, with very slight variations, all the youth of the present day is grouped. Is this for good or for evil? Far be it from me to open such a discussion here; I merely say that such is the fact, and this fact will explain why I have collected various particulars of the childhood of our colleague, which might otherwise have appeared trifling.

Carnot was only ten years old when his mother, in a journey to Dijon, took him with her, and, to reward him for the thoughtful docility which he always showed, took him to the theatre. A piece was represented that day, in which evolutions of troops and battles succeeded one another without intermission. The young scholar followed with sustained attention the series of events which were developed before him; but, all on a sudden, he gets up, he is agitated, and, in spite of the endeavours of his mother, calls out in terms hardly polite to an actor who had just come on the stage. This person was the acting general of the troops on whose side the young Carnot was interested; by his cries, the child was warning the unskilful chief that the artillery was badly placed; that the gunners, being without cover, must necessarily be killed by the first fire of musketry from the ramparts of the fortress which they were about to attack; that, on the other hand, by establishing the battery behind a rock, which he pointed out, both by word of mouth and by gesture, the men would be much less exposed. The astonished actors did not know what to do; Madame Carnot was

distressed at the disturbance which her son was occasioning; th audience burst out laughing; every one was puzzled as to th cause of such an unusual criticism; and the supposed frolic wa nothing else than the revelation of a superior military talent, th first symptom of that powerful genius which, despising beate tracks, created a few years later new tactics, and proposed to re place the scientifically and ingeniously combined fortifications o Vauban, by an altogether different system.

From the age of twelve to fifteen, Carnot pursued the cours of studies at the College of Autun. He made himself remarkabl there by a lively, original turn of mind, and by a rare degree o intelligence. He next entered the "little seminary" of the sam town. At sixteen years of age he had finished his Philosophy. The firmness which we shall find in him in the course of a mos stormy career, was already the leading feature in his character. The timid professors of the seminary of Autun, had a troublesom experience of it on the day when their scholar had to support hi thesis.

This ceremony always took place in public. According to regulations, the liberality of which would, at the present day, appea excessive to the authorities of our universities, every one of th audience had the right of making objections. This criticism migh be applied both to the principles and to the style. Thus the *amou propre* of the master ran as much risk as that of the pupil, and the reputation of a large establishment lay at the mercy of som heedless young fellow. Thence came the custom of starting th competitors in the arena accompanied by a Mentor, who came t the assistance of their treacherous memories, and who, by a word put in at the proper moment, brought them back into the righ path as soon as they began to wander from it; and the Mentor wa often himself drawn into the discussion on his own account. According to this custom, the teachers of the Seminary of Autun were proceeding towards the *salle des exercises*, where a large concourse of people was assembled, when the young Carnot signified his intention to ascend to the rostrum alone, that he would not be accompanied by a prompter, that he would not keep at all to the routine they had assigned him, and that he would speak alone or not all. This resolution was combated by alternate entreaties and threats, but in vain: they were obliged to submit, whether they liked it or not, to this unprecedented caprice of the pupil. However, the most brilliant success soon justified it, even in the eyes of the irritated professors. A curious incident rendered the meeting remarkable: a lady, the wife of a doctor of medicine, became the most formidable adversary of the young rhetorician;

she argued against him, in Latin, with a force of logic, with an ease, a grace, and an elegance of expression, which the more astonished Carnot and the audience, inasmuch as no indiscreet display had hitherto made them even suspect that Madame l'Homme had carried her studies farther than the *Cuisinière bourgeoise*, the *Almanach de Liége*, or the *Petit Paroissien*.

Carnot had so thoroughly taken, not only to the principle of religion, but, moreover (and they are not the same things), to the minute practices of devotion scrupulously followed at the little seminary of Autun, that some of his friends thought at one time of putting him into holy orders. They were strengthened in this idea by the recollection of the great number of ecclesiastical dignitaries of which this honourable family could boast, amongst whom figured canons, *vicaires-généraux* of the diocese of Châlon, doctors of the Sorbonne, and an abbé of Cîteaux. However, the career of military engineer carried the day, and young Carnot was sent to Paris to a special school, there to prepare for his examination. The comrades whom he met there had certainly not been brought up at the Seminary; for the profound piety of the new scholar, of which he would by no means make a mystery, became the subject of their continual sarcasms. Sarcasms are not reasons. Carnot was not therefore staggered by them; but he felt the necessity of maturing, by reflection and study, ideas and sentiments to which his pure and candid soul had hitherto given itself up with perfect good-will and confidence. Theology, then, became, for some months, the only occupation of an *apprenti-officier*, or military novice. No one can tell what was the effect of these meditations; for, at all periods of his life, Carnot carefully avoided, even in the intimacy of the domestic circle, any discussions — nay, more, any simple conversations—relating to religion. We only know that he professed principles now adopted by all good and enlightened minds. "Universal tolerance," said he, when, proscribed and wandering in a foreign land, he had to ward off the spiteful darts of calumny,—"universal tolerance, that is the dogma which I decidedly profess . . . . I abhor fanaticism, and I believe that the fanaticism of irreligion, brought into fashion by such men as Marat and Père Duchêsne, is the most fatal of all. We must not kill men to force them to believe: we must not kill them to prevent their believing; let us compassionate the weaknesses of others, since every one has his own; and let us allow prejudices to wear away by time when we cannot obviate them by reason."

After theology, scientific studies, especially those of geometry and algebra, had their turn, and, as at Nolay and Autun, his success was rapid and brilliant. M. de Longpré, director of the preparatory school, was acquainted with D'Alembert. The illustrious

geometer was not above going amongst very young scholars, to encourage rising merit by his approbation. In one of his visits he particularly distinguished Carnot, and addressed to him flattering and prophetic words, which our colleague would repeat with emotion, even during those periods when fortune had rendered him one of the arbiters of the destinies of Europe.

Perhaps this is an opportunity, Gentlemen, for regretting that, in our society, such as half a century of revolutions has made it, the personal intercourse which formerly existed between the professors and distinguished scholars of great schools, has totally disappeared, and has become indeed, to a certain degree, impossible. Now-a-days, at the hour set down in the programmes, illustrious men of learning or of literature arrive in spacious amphitheatres. A crowd is waiting for them. During entire hours, all that is profound, intricate, or new, in science or literature, is developed with system, clearness, and eloquence; but, the lesson finished, the professor retires, without even knowing the names of those who have listened to him. Nevertheless, in the midst of such an audience (I will confine myself, Gentlemen, to a single example), Fourcroy found, in an apothecary's boy who had come furtively to hear him, the devoted, exact, indefatigable, and ingenious co-operator whom, by these traits, each of you has already recognised — he discovered Vauquelin!

ENTRANCE OF CARNOT INTO THE SCHOOL OF MÉZIÈRES AS SECOND LIEUTENANT OF ENGINEERS.

At the time when Carnot quitted the establishment of M. de Longpré, the "ordonnance" in virtue of which a genealogist co-operated with a geometer in the examination of the future officers of engineers was not in force. In 1771 any Frenchman might still be admitted at the school of Mézières without showing any parchments, on condition always that neither his father nor mother had endeavoured to enrich their family and their country by commerce or by manual labour. The young aspirant displayed unusual mathematical knowledge before the examiner, Bossut. His father, in obedience to the sad exigencies of the period, proved on his part that no ship of his had ever been to distant countries to exchange the fruits of the French soil or of French industry, for productions reserved by nature to other climates; that his hands had never put together the movable types of Gutenberg, even for the purpose of reproducing the Bible or the Gospel; that he had not personally co-operated in the construction of any of those admirable instruments which measure time, or which sound the depths of space.

After legal proof of these negative merits, young Carnot was declared of sufficiently good family to wear an epaulette, and received without delay that of a second lieutenant.

Decorated with this so-much-desired epaulette, Carnot, at the age of eighteen years, came to the School of Engineers. There, under the auspices of Monge, he doubtless cultivated descriptive geometry and the physical sciences with his habitual success; but on this point it must be owned, we are reduced to mere conjecture; for, in carrying to an extreme the natural desire to conceal from strangers the knowledge, then but little spread, of the art of making and destroying fortifications, the celebrated school of Mézières had been made a sort of conclave of which the secrets were never penetrated by the profane.

CARNOT A FIRST LIEUTENANT ON SERVICE IN FORTRESSES.

On the 12th of January, 1773, Carnot, having become a first lieutenant, was sent to Calais. The works of a place where the periodical oscillations of the ocean add a new and important condition to the already very complicated data of the problem of fortification, were very interesting to the young officer. He thus overleaped without hindrance, the passage, generally so troublesome, from learned theories to tiresome practice; from the brilliant illusions which amuse us in schools, to the sad realities of life.

The *Mémorial de Saint Hélène* says that in his youth "Carnot was looked on by his comrades as an original." This title Napoleon had borrowed from Carnot himself. I find it in the answer to Bailleul, but explained and commented on, and deprived of that vagueness which leaves it to be taken either as a compliment or a reproach. Carnot, at twenty years of age, was, to the officers of the garrison of Calais, an "original," or a "philosopher," (these words were equivalent) because he did not join them either in their turbulence or in any of their wild pranks; because he passed his time in the libraries rather than at the café; because he read Thucydides, Polybius, and Cæsar, rather than the licentious works of that period; because, if he were intimate with the Prince de Croy, Commandant-General of Picardy, it was not for the sake of obtaining leave from, or alleviations of, duty, but in order to assist him in delicate geographical researches, and to work at charts of the Southern Hemisphere showing the latest nautical discoveries. Carnot, nevertheless, was anything but an ill-natured judge of others. Severe towards himself, he had an inexhaustible fund of indulgence to everyone else. He employed his hours of leisure or relaxation in composing little poems, all impressed with a gentle and social gaiety. To have quoted

ballads in the biography of a geometer would certainly have had great novelty, and this weak merit, quite within my grasp, had almost persuaded me to do so; a little reflection has caused me to give it up. A great poet in our country having stamped that nature of composition with his immortal seal, song should no longer be lightly quoted.

THE FIRST COMMUNICATION BETWEEN CARNOT AND THE ACADEMY OF SCIENCES. — AIR-BALLOONS.

The first direct communication between Carnot and the Academy of Sciences (this fact will be a novelty to everyone) was brought about by a problem which not only has not yet been solved, but which, according to many physical philosophers, appears as if it never can be —" the problem of guiding balloons."

Scientific discoveries, even those from which mankind might expect the greatest advantages — such, for instance, as those of the mariner's compass and the steam-engine — were received on their first appearance with disdainful indifference. Political and military events exclusively enjoy the privilege of exciting the public. There have been, however, two exceptions to this rule. You will all know by this hint, that I allude to America and air-balloons, Christopher Columbus and Montgolfier. The discoveries of these two men of genius, so different hitherto in their results, had, at their birth, similar fortunes. Gather, in fact, from the *Historia del Almirante* the marks of the general enthusiasm which the discovery of certain islands excited amongst the Andalusians, the Catalonians, the Arragonese, and the Castilians; read the account of the unheard-of honours which they hastened to render, as well in the largest cities as in the smallest hamlets, not only to the leader of the enterprise, but even to the very sailors of the caravels La Santa Maria, La Pinta, and La Niña, which were the first to reach the western shores of the Atlantic; you may then save yourselves the trouble of searching in the writings of the period what sort of sensation air-balloons produced amongst our compatriots: the processions at Seville and Barcelona were faithful representations of the fêtes which took place at Lyons and Paris. In 1783, just as it happened two centuries before, warm imaginations were not at the trouble of confining themselves to the limits of facts or of probabilities. In the one instance, there was not a Spaniard who did not wish, after the example of Columbus, himself also to tread lands where, in a few days, he might collect as great a quantity of gold and precious stones as was formerly the possession of the richest potentates. In France, each individual, following the favourite direction of

his ideas, made different but charming applications of the new faculty—I had almost said of the new organs—which man had just received from the hands of Montgolfier. The physical philo sopher, transported into the region of meteors, and catching Nature in the act, penetrated at a glance the mystery of the formation of lightning, of snow, and of hail. The geographer, profiting by a favourable wind, was to explore, without danger or fatigue, as well those polar zones which the accumulated ice of centuries seems to wish to conceal for ever from our curiosity, as those central parts of Africa, New Holland, Java, Sumatra, and Borneo, forbidden to our enterprises not less by a deadly climate than by the fierce animals and tribes which live there. Certain generals thought it an urgent duty to study the systems of fortification and artillery which it would be necessary to oppose to enemies moving in balloons; others elaborated new principles of tactics applicable to aërial battles. One would say that projects such as these, which might have been fathered on Ariosto, should certainly have satisfied the most adventurous and enthusiastic spirits: such was not the case however. The discovery of balloons, notwithstanding the brilliant accessories with which each one enthusiastically surrounded it, appeared to be only the forerunner of still greater discoveries; henceforward nothing was to be impossible to one who had conquered the atmosphere. This idea was continually reproducing itself; it put on every shape; youth seized it with joy; old age made it the text of a thousand bitter regrets. See the Maréchale de Villeroi, an octogenarian and an invalid: she is led to one of the windows of the Tuileries almost by force, for she does not believe in balloons; the balloon nevertheless detaches itself from its moorings; our colleague, Charles, seated in the cradle, gaily salutes the spectators, and soars majestically into the air. Oh! on the instant passing without transition from the most complete incredulity to an unbounded confidence in the powers of the human mind, the old Maréchale falls on her knees, and, her eyes bathed in tears, gasps forth these sad words: "Yes, it is decided, now it is certain; THEY will discover the secret of never dying, *but it will be when I am dead!*"

Carnot, being of a rigorous turn of mind (though he was not yet eighty years of age), took good care not to go so far as the Maréchale de Villeroi. Nevertheless he appeared in the ranks of the enthusiasts. He then believed, and always did so afterwards, in the possibility of directing balloons, and consequently in the applications which science and the art of war had hoped from them. The archives of the Academy ought to contain a paper in which Captain Carnot of the engineers submitted to the authorities an arrangement of light oars, which, in his opinion, should attain the

desired end. This paper has not yet been discovered. I will continue my researches for it, and if the work seems likely to add to the reputation of our fellow academician, the public shall not be deprived of it. Perhaps I shall join with it a memoir of the same nature, also unpublished, by another academician, the illustrious Meunier.

ÉLOGE OF VAUBAN BY CARNOT.—HIS DISCUSSIONS WITH M. DE MONTALEMBERT.

A certain literary society of a very small town once on a time gave itself the title, on its own full authority, of *Daughter of the French Academy.* Voltaire thought that they should not refuse it this title: "Indeed, I esteem her," said he, "as a very virtuous daughter, since she has never given occasion for any talk about her."

Such an epigram would not have been applicable to the Academy of Dijon. This celebrated society did not shun the public gaze, either when it proposed the question, "Whether the re-establishment of the arts and sciences had contributed to the refinement of manners," nor, more especially, when it rewarded the discourse in which Jean-Jacques pronounced in the negative. Time has done ample justice to the paradox; but it ought not to have effaced the remembrance of the generous proceeding which, in giving to Rousseau an unexpected celebrity, attached him for ever to the brilliant career in which he met with competitors and rivals, but not with a master.

To the merit which I have just related, the Academy of Dijon can add that of having called forth the first production of Carnot's which the press took possession of,—the *Eloge of Vauban.*

The intrepidity, the disinterestedness, and the science of the illustrious marshal had already received, from the tongue of Fontenelle, an homage to which it seemed difficult to add. What speech indeed could more worthily characterise a military life than these few figures? "Vauban caused work to be done at 300 fortresses; he constructed 33 new ones; he conducted 53 sieges; he was present at 140 actions of importance." And does not this other sentence seem as though borrowed from Plutarch? "The morals of Vauban held out perfectly against the most brilliant dignities, and never even wavered. In a word, he was a Roman whom it seemed as if our age had stolen from the best times of the Republic!"

The éloge from which these two passages are taken had always appeared to me so eloquent and true, that, at the moment when I first discovered an oration on Vauban amongst the pro-

ductions of our colleague, I burst out into heartfelt abuse at the academic programme which, taking advantage of the inexperience of a young man, had exposed him to so formidable a comparison. Indeed, I should not have been more uneasy, if I had discovered that Carnot had endeavoured to re-write *La Mécanique* of Lagrange, *Athalie*, or the *Fables* of La Fontaine. These fears were superfluous. The Burgundian members of the Academy of Dijon were right in thinking that the Burgundian Vauban might still become an interesting subject of study, even after the brilliant portrait traced by Fontenelle. And, in truth, the Secretary of the Academy of Sciences had prudently left in the shade one of the finest points of the illustrious marshal.

It would seem that the éloge of Vauban, from the pen of an officer of engineers, must consist principally of an exact appreciation of the means of attack and defence with which the illustrious marshal endowed the art of war. This was not the plan, however, which Carnot adopted. It was principally for the qualities of the heart, for virtue, and for patriotism, that Vauban seemed to him worthy of admiration. "He was," said he, "one of those men whom nature gives to the world formed entirely for benevolence; gifted, like the bee, with an innate activity for the general welfare; who cannot separate their lot from that of the Republic, and who, intimate members of society, live and flourish, or suffer and languish, with it."

Prince Henry of Prussia was present at the assembly of the Academy of Dijon, at which the éloge of Vauban was read and rewarded. He expressed, in the most unequivocal terms, the great pleasure that the discourse had given him; and assured the author of his profound esteem, both verbally and in writing. Piqued with emulation, the Prince de Condé, who presided at the assembly, as governor of Burgundy, outdid the marks of favour which were shown to the young engineer officer by the brother of Frederick the Great.

Had Carnot then flattered the prejudices of the nobles? Were his principles in 1784 so different from those which afterwards directed all his actions, as necessarily to receive the suffrages of the great? Listen, Gentlemen, and judge!

The *Dîme Royale* (the King's Tithe), that writing which, under Louis XIV., brought about the complete disgrace of Vauban, and of which Fontenelle had the prudence not even to mention the title, in enumerating the works of the illustrious marshal, was called by Carnot a simple and pathetic exposition of facts; a work in which "everything is striking by its precision and truthfulness." The assessment of the taxes, in France, in the eyes of the young officer, was "barbarous;" the manner of

gathering them "more barbarous still." According to him, the true object of a government is to oblige every individual of the State to labour; the method which he points out for obtaining this result would be (I quote from the text) to cause riches to pass from those hands where they are superfluous, into those where they are necessary. Carnot gives his adhesion unreservedly to this precept of Vauban's; the laws ought to prevent the frightful misery of the one class and the excessive opulence of the other; he sets his face against the odious multiplicity of privileges from which the more numerous classes of the population had then so much to suffer; finally, after having divided mankind into two categories, the workers and the idlers, he goes so far as to say of these latter, who alone, according to him, have been taken into account in the constitution of modern society, that "they do not begin to be useful till the moment in which they die, for they do not vivify the earth except by re-entering it." Such, Gentlemen, are the bold opinions which an Academy rewarded in 1784; which called forth from Buffon, who certainly cannot be accused of having been an innovator in matters of government, these words so flattering to the successful orator:—"Your style is noble and flowing; you have done, sir, an agreeable and useful work;" and which inspired the brother of an absolute king with the desire of attaching Carnot, whose "friend" he declared himself to be, to the service of Prussia; which gained for the young officer the favour of the prince whom Worms and Coblentz witnessed a few years afterwards at the head of the emigration! Who then will dare to call our revolution of 1789 an effect without a cause, a meteor of whose arrival there had been no warning? The moral transformations of society are subjected to the law of continuity; they rise and grow like the productions of the earth, by imperceptible gradations.

Each century developes, discusses, and adapts to itself, in some degree, truths—or, if you prefer it, principles—of which the conception belonged to the preceding century; this work of the mind usually goes on without being perceived by the vulgar; but when the day of application arrives, when principles claim their part in practice, when they aim at penetrating into political life, the ancient interests, if they have only this same antiquity to invoke in their favour, become excited, resist, and struggle, and society is shaken to its foundations. The tableau will be complete, Gentlemen, when I add that, in these obstinate conflicts, it is never the principles that succumb.

Carnot, as I have already remarked, had but lightly touched on the technical part of Vauban's works, in his éloge; yet, in the few sentences which he wrote on this subject, he took occasion to say that a "*a certain vulgar ignorant person*" took an erroneous

view of fortification in reducing it to the art of tracing on paper lines subjected to certain, more or less, systematic conditions. These words, in their general sense, seemed as if they might have passed unnoticed; but an unfortunate concurrence of circumstances gave to them an importance which was not foreseen, and still less desired by their author. In 1783, a general of infantry, member of this Academy, M. le Marquis de Montalembert, published, under the title of *Perpendicular Fortification*, an entirely new system of defence of fortresses. This system was outrageously opposed by almost the whole corps of military engineers. The scion of an illustrious family, the general officer of the French army, the academician, might assuredly, without too much vanity, believe himself not included in the *ignorant vulgar* that the author of the eulogy had lightly designated; but M. de Montalembert was determined to apply these expressions to himself, and to revenge himself he published an edition of Vauban's éloge accompanied by notes, in which offence and gross affront were carried to the utmost. There was enough in this pamphlet to upset the mind of a young man a thousand times; nevertheless, under these difficult circumstances, Carnot already showed himself such as he always was afterwards — frank, just, and completely insensible to undeserved abuse.

"If your suspicions were well founded," wrote he to his fiery antagonist, "I should have forgotten the first duties of propriety and decency; I should have been wanting, above all, in the infinite respect which military men owe to a distinguished general: be assured that there is not a single officer of engineers who has not learnt with the same pleasure, from M. le Marquis de Montalembert, how to fortify places well, as from the brave D'Essé to defend them well."

The appositeness and delicacy of this quotation will be appreciated when I mention that the brave D'Essé, who, in 1543, after three months of an heroic resistance, compelled the whole forces of the emperor to raise the siege of Landrecies, was an ancestor of M. de Montalembert.

Moderation and politeness are almost infallible means of success against violence and affront; moreover, in the quarrels of the press, they must often be looked upon as the simple result of calculation, and as proofs of ability. But Carnot's letter allowed no misapprehension as to the sincerity of his sentiments. "Your work," he wrote to him who had just criticised so bitterly the principle, the style, and I might almost add, the punctuation, of his éloge, "your work *is full of genius* . . . . *Now that your casemates are known and proved, fortification will put on a new face; it will become a new art.* It will be no longer allowable to employ the

revenues of the State to construct something tolerable, when you have taught us to construct something good. . . . . Although the corps of engineers has not the advantage of possessing you, we do not the less consider that we have a right to reckon you amongst its most illustrious members. Whoever extends our knowledge, whoever furnishes us with new means of being useful to France, becomes our comrade, our chief, and our benefactor." M. de Montalembert did not resist such explicit and flattering testimony. The most formal disavowal of the unlucky pamphlet quickly followed Carnot's answer; on the other hand, it must be confessed that the higher authorities of the engineers were so irritated at the praises which a simple captain had allowed himself to bestow on systems which they had authoritatively rejected, that a "lettre de cachet" and the Bastille signified to our member that, on the eve of our great revolution, liberty of discussion, that precious conquest of modern philosophy, had not yet penetrated amongst military usages. Such rigour seems inexplicable, even when one makes every allowance for the requirements of *esprit de corps* and the susceptibilities of self-esteem; Carnot had shown himself, indeed, both in his éloge and in his letter to Montalembert, the warmest defender of the department to which he belonged, and which, said he, "professes to sacrifice its time and its life for the State." Had this man then, I demand, forgotten the duties of his position, who, when called on to judge between the services of a regimental officer and those of the engineer on whom devolves the dangerous honour of tracing parallels, of commanding in the trench, or of directing the head of a sap, expressed himself so nobly: "The officer of engineers is in the midst of peril, but he is there alone and silent; he sees death, but he must gaze on it with coolness; he may not rush on it like the heroes of battle; he sees it approach with tranquillity; he seeks the spot where the lightning bursts forth, not to act, but to observe; not to get excited, but to deliberate."

Perhaps, Gentlemen, I should not have insisted at such length on this painful episode in Carnot's life, if I had not had opportunity of perceiving how far removed are such times from ours; if I had not seen, when accompanying our most illustrious officers of engineers in the inspection of some fortified towns, in the discussion of the amelioration they might be susceptible of, the simple *sous-lieutenant* freely oppose his ideas, reflections, and systems, with full liberty, to the opinions of the generals; surrender only after having been victoriously refuted; and come forth from the animated contest, not, as formerly, to go to the Bastille, but with fresh chances of advancement.

Those on whom the duty devolves of incessantly referring to

the ameliorations of which our social state is susceptible, would become discouraged, Gentlemen, if, when occasion presents itself, we did not show the public that their endeavours have been sometimes crowned with success.

ESSAY ON MACHINES. — NEW THEOREM ON THE LOSS OF POWER.

The first — nay, more, the principal — scientific production of Carnot, bears the date of the year 1783: it is entitled *Essay on Machines in general.*

They who would seek in the essay of our member the technical description or special study of any one of the machines in particular, simple or composite, from which man has been able to derive so many advantages, would labour to no purpose. Such was not, indeed, the end which the author had in view.

A machine, considered generally, is the assemblage of a more or less considerable number of fixed or movable pieces, by the aid of which forces of all sorts ordinarily produce effects which their direct action could not bring about. Take, for instance, the stonemason with his hand on the handle of a very simple machine, the winch of the lifting-jack or the roller: he turns about enormous blocks, or inclines them to suit his convenience, or raises them to the summit of the highest buildings, when, without the machine, he could not stir them a hair's breadth.

At sight of these effects, the ignorant make great outcry at the marvel: they persuade themselves that machines multiply force, and this false idea, radically false, leads them into fantastic and generally very complicated conceptions, which take away an immense quantity of capital every year, in pure loss, from agriculture, and manufacturing industry, and commerce.

With a force of any nature whatsoever, that which must be valued in money, that which the fabricator buys from the engineer, may be easily referred to a very simple effect, of which everyone has a clear idea. Let the force be supposed directly applied to the raising of a weight: the height to which the force raises the weight in a certain time is observed, and these two data from experiment, the weight and the height, multiplied together form a product which is the exact value of the force employed. This product, indeed, for a given time and the same height, cannot be augmented or diminished without the force augmenting or diminishing in the same proportion; so that, for example, if it becomes double, triple, or decuple, it is the result of the force being multiplied by two, three, or ten.

The product, which gives the direct measure of a force, serves equally to measure it when it exercises its action against a

resisting body by an intermediate machine; well, endow this machine, if you please, with the power of thought and all the perfections imaginable, and the product of the weight multiplied by the height it will have passed through in a given time, will be exactly equal to that obtained by the employment of the same force without any intermediate operation. The real effect, then—or, to speak more strongly, the effect of any machine when properly estimated—will never surpass that which the motive force was capable of producing naturally. Doubtless you can, if you like, with a machine, raise enormous masses, of millions or thousands of millions of pounds; but since the product of the weight, multiplied by the height, must remain constant, the height to which these masses can be raised in a minute will be millions or thousands of millions of times smaller than that to which your hand might have raised one pound in the same time.

Everyone will now understand the true meaning of that aphorism in mechanics, Machines lose in time or velocity what they gain in power! Give me a point of support situated outside the earth, cried Archimedes, and I will, with the aid of a lever, raise this earth, so large and massive, by the mere effort of my hand. The exclamation of the immortal geometer was marvellously characteristic of machines, in so far as they give to man the means of realising effects otherwise millions of millions of times beyond his natural strength; but antiquity would no doubt have admired it much less if any one, analysing phenomena more closely, as we have just done, had added: Yes, doubtless, mathematically speaking, with his fulcrum and lever Archimedes might raise the globe; but, after forty million centuries of continuous effort (for such a calculation is not, at the present day, beyond the limits of science), the movement effected would be hardly the breadth of a hair.

If the ideal machine, the machine endowed with all imaginable perfections, adds nothing to the force which puts it in action, at any rate it takes nothing away from it; it transforms the effects by rigorous equivalents. It is not thus with a real machine; in this case the power and the resistance communicate with one another by means of pieces which we had supposed inflexible, and which are not so; by means of chains and cords whose roughness cannot but be injurious; the movable parts, moreover, turn in collars or sockets where great friction takes place: all these causes united absorb in pure loss a very noticeable part of the motive force; so that the effect of a machine must always be inferior to that which would have been engendered by the power acting directly on the resistance.

These results of theory, which are, moreover, completely con-

firmed by experience, yet allow that, under certain points of view some particular machine may be recommended without paradox; that it may be useful, and often even indispensable. For instance, considerations of solidity or ornament necessitate the carrying to the summit of certain edifices blocks of stone or marble whose weight is beyond the strength of the most vigorous workman; suppress the windlass and analogous machines, and one man will no longer be able to execute the work which the architect has conceived; it will be necessary to unite the strength of thousands of arms at the same point; the narrowness of space will prevent that; the character of grandeur will disappear from all the monuments of architecture; the triumphal arch, the palace, will only be constructed, like the humble cottage, of little rough stones.

You see, Gentlemen, that there are cases, it cannot be too often repeated, in which we must resign ourselves, whether we will or no, to the loss of force consequent on machines, since, without their help, certain works would become impossible.

The losses of force which depend on the flexibility of the materials of which machines are composed, on the roughness of cords, and on friction, had been remarked by the most ancient mechanicians; modern ones have gone farther; their experiments enable them to appreciate these losses and value them in numbers with tolerable exactness. Science had arrived thus far, when Carnot published his Essay. In this work, our member, looking on machines, and even more generally on every system of movable bodies, from an entirely new point of view, indicates a cause unperceived, or at any rate imperfectly analysed, by his predecessors, and which in certain cases must also give rise to considerable losses; he shows that we ought, by all means, to avoid abrupt changes of velocity. Carnot does more; he finds the mathematical expression of the loss of active force which such changes occasion; he shows that it is equal to the active force by which all the various bodies of the system would be animated, if each of them were endowed with the complete velocity which it lost at the instant of the abrupt change being effected.

Such is, Gentlemen, the enunciation of the principle which, under the name of "Carnot's Theorem," plays so great a part in the calculation of the effect of machines.

This beautiful and valuable theorem is now well known to all engineers; it guides them in practice, and secures them from the gross faults committed by their precursors.

To give an idea of its importance to the generality of the world, I should be inclined to say, notwithstanding the fantastic appearance of the comparison, that Carnot has extended to the material world a proverb whose truth was only established, before

his time, in the moral world; that "much noise* and little work" is a saying henceforth quite as applicable to the effective labours of machines, as to the enterprises of certain individuals whose petulance gives rise to the hope of wonders destined not to be realised. In addressing men of learning, I would beg them to distinguish carefully between the invention of the material organs by whose aid forces transmit their action from one point to another, and the discovery of those primordial truths which are applicable indistinctively to all imaginable systems; I will endeavour to show that in this first respect the ancients were perhaps not inferior to us. The screw of Archimedes, the series of toothed wheels of Ctésibius, the hydrostatic fountains of Heron of Alexandria, the steam rotating machine of the same engineer, a great number of warlike machines, and amongst them the balista, might all be brought forward to strengthen my view. In the field of theoretical truths, on the contrary, the preponderance of the moderns would show itself incontestable.† There we should see successively, in all their brilliancy, in Holland, Stévin and Huyghens; in Italy, Galileo and Torricelli; in England, Newton and Maclaurin; in Switzerland, Bernouilli and Euler; in France, Pascal, Varignon, D'Alembert, Lagrange, and Laplace.

Well, Gentlemen, those are the illustrious personages amongst whom Carnot made a place for himself by his beautiful theorem.

Perhaps, indeed, I ought to be afraid that, by insisting any longer on the inconvenience of abrupt changes, I may inspire my audience with the desire that I should, notwithstanding every inconvenience, pass "abruptly" to another subject; nevertheless, I will hazard a few more words.

We have just been talking frequently of lost force; the expression is correct when we compare the actual effect of a machine with that which it might have produced if, all other circumstances remaining the same, the constructor had carefully avoided sudden changes of speed; but it must not be imagined that any force, or fraction of a force, can be ever annihilated, in the grammatical acceptation of the word; all that which is not found in the useful effect produced by the motive power, nor in the amount of force

* The proverb does not fit at all neatly, unless "noise" be taken to mean "irregularity;" some good machines are very noisy.—*Translator.*

† The question is rather unfairly stated against the ancients; for Arago speaks as if Archimedes, &c., had only *made* their machines, and not been masters of the principles, which involved as much primordial truth as any other discoveries. A fairer distinction seems to be, that the moderns launched out into realms where *theory* alone could point out the way; the ancients were led on by *experiment* and *observation.*—*Translator.*

which it retains after having acted, must have gone towards th shaking and destroying of the machine.

This last remark was necessary for the appreciation of th eminent and incontestable services which Carnot's theorem ha already rendered and will render more and more to art and in dustry. If I were not afraid of the incredulity which would, a first sight, attach itself to my words, I would add that this sam theorem of analysis and mechanics has also played a great part i the numerous events of our Revolution, whose character Carnot' determinations were able to change. However, I have said to much not to complete the idea.

In my youth, encouraged by the good-will and friendship wit which Carnot was kind enough to honour me, I sometimes took the liberty of calling his recollection to those great epochs of ou revolutionary annals, when parties, in their frenzied convulsions, were destroyed, conquered, or merely appeased, by abrupt and violent measures, by real coups d'état. Then I would ask ou colleague how he, alone amongst all the others, had constantly hoped to arrive at the goal without shocks, and without infringement of the laws; his answer, always the same, had become deeply graven on my memory; but what was my surprise when, emerging one day from the round of studies which a young astronomer should always impose on himself, I found, word for word, this constant answer which we have just been discussing in the enunciation of a theorem of mechanics; when I saw that our colleague had always discoursed with me on the political organisation of society precisely in the same manner as he speaks in his work of a machine, in which abrupt changes necessarily involve great losses of force, and sooner or later bring about the complete dislocation of the system! *

Can it then be true, Gentlemen, that in the weakness of the human race, the loftiest spirits have been so little convinced of the goodness and truth of the determinations which their hearts inspire them with, that they have found it necessary to confirm and corroborate them with more or less forced assimilations?

This doubt will not astonish you if I add that one of the learned men whose works have conferred the greatest distinction on this Academy, conducted himself on all difficult occasions (so we are to believe), according to the following assuredly very convenient maxim; "Water takes exactly the form of the vase which contains it; a wise mind should as faithfully model itself on the circumstances of the moment."

* This parallel cannot be deemed exact: in the Revolution, they wanted to destroy one machine altogether, and supply quite another; so the rules applicable to steady machinery, or government, do not apply. — *Translator.*

I might quote also another of our colleagues equally celebrated, of whom a certain personage asked one day in my presence, by what secret he had passed through the terrible periods of our civil discords without mishap: "Every country in a state of revolution," answered he, "is a carriage of which the horses have taken the bit between their teeth; to wish to stop the horses is to rush on a catastrophe from gaiety of heart; he who leaps from the carriage exposes himself to being crushed under the wheels; the best plan is to abandon one's self to the movement, and shut one's eyes; so did I!"*

In the work whose analysis has carried me farther than I expected, Carnot has devoted some lines to the question of perpetual motion! He shows not only that every machine, of whatever form, abandoned to itself will stop, but he moreover assigns the moment at which that must happen.

The arguments of our colleague are excellent; no geometer will dispute their exactness; may we yet hope that they will nip in the bud the numerous projects which every year, or rather "every spring," sees burst into flower?

This is what we cannot hope for. The contrivers of perpetual movements would no more comprehend the work of Carnot, than the discoverers of the quadrature of the circle or the trisection of the angle understand the geometry of Euclid.† Science is not needed by them; they owe their discovery to a sudden supernatural inspiration. Moreover, nothing discourages them, nothing undeceives them; take for example that artist, otherwise highly estimable, who, without perceiving anything innocently burlesque in the terms of his request, begged me to go and see "*why all his perpetual movements had stopped.*"

CARNOT A POLITICIAN AND ONE OF THE JUDGES OF LOUIS XVI.

Carnot was one of the first officers of the French army that loyally and enthusiastically embraced the regenerative views of the National Assembly. Nevertheless, the annals of the Revolution only commence making mention of him in 1791.

Certain writers wrongly take the spirit of proselytism as the just measure of the sincerity of political convictions; they do not understand how a retired and studious life may ally itself to a profound desire for social reforms; Carnot's two years of inaction

* If the horses could not be stopped, surely an attempt should be made to guide them.—*Translator.*

† Not quite a just comparison. There is no reason why these geometric feats *must* be impossible, as is the case with perpetual motion.—*Translator.*

seem to them quite a phenomenon. Now, guess how they deemed it advisable to explain it. They place our member amongst the émigrés of Coblentz; thus his republican tendencies would only date from the period at which he furtively re-entered France. I will not offend you, Gentlemen, by refuting such a ridiculous supposition.

In 1791 Carnot was in garrison at Saint-Omer, and there married Mademoiselle Dupont, daughter of a military administrator born in that country. His political principles, the moderation of his conduct, and his varied knowledge, shortly procured him the honour of representing the department of the Pas-de-Calais in the Legislative Assembly. From this period Carnot gave himself up entirely to the imperious duties which were imposed on him, either by the choice of his fellow-citizens, or the voice of his colleagues. The public character almost entirely absorbed that of the geometer: this last only showed itself henceforth at long intervals.

Here, Gentlemen, two roads present themselves to me; one is smooth and open, the other bordered by precipices. If I listened to some persons whose good will towards me has rendered them timid, I should not hesitate to choose the first. To take the other would be to incur, I am well aware, the reproach of imprudence and blindness. Heaven keep me from supposing that I am strong enough to struggle against such clear and decided opinions; but wretched considerations of self-love will always vanish from my sight before the sentiment of duty. Now, I ask, should I not deeply wound the public conscience if, in this area consecrated to the arts, letters, and sciences, I confined myself to speaking of Carnot as an academician? Without doubt one might, whilst developing before you the long series of discoveries of this or that illustrious savant, endowed during his life with the title of senator, legitimately—very legitimately—cry out that posterity would not preserve any recollection of functions without effect, and which, moreover, descending from one degradation to another, had ended by reducing themselves to a monthly communication with the treasury; but it would be an antinational and ungrateful act to apply such words to the great shade of Carnot. I am desired, wished, and almost ordered to do this. Well! I consent, I will not speak of the drama whose "dénouement" was the tragic death of the successor of a hundred kings, and the overthrow of the monarchy; nevertheless I, a decided partisan of the abolition of the punishment of death, do not perceive the supposed difficulties of position which should have hindered me from abandoning myself here publicly to the inspirations of my conscience; nor do I see any better, why I should have abstained from rendering this assembly aware of the deep aversion which I profess for every

political decree issued by a political body. Must I say it, in a word? —a fraternal solicitude for the memory of Carnot did not appear to me to require the sacrifice which is imposed upon me. Is it forgotten how contemporaneous history would have furnished me with accusing documents against the thousand courtiers whose interested, hypocritical, and antinational manœuvres cast the monarch into a labyrinth without exit, caused him to be unanimously declared culpable by the national representatives, and were much more instrumental than the ardent democratical ideas of the Convention in rendering the catastrophe of the 21st of January inevitable? If from these high moral considerations I had descended to a minute appreciation and technical discussion of facts, such as one has to submit to a court of appeal or of repeal, I should, in company with all upright minds—with our Daunou, for example,—have found the illegality of the celebrated trial, less in the nature of the sentence, less in the severity of the punishment inflicted, than in the very composition of the tribunal, or in the usurpation of power which had given birth to it. Now, Gentlemen,—and this is a point I should not have failed to remark on,—when the Convention was investing itself with the right of pronouncing on the fate of Louis XVI.; when after this stroke it was regulating its jurisprudence; when it was simultaneously attributing to itself the functions of accuser and judge, Carnot was absent from Paris; he was fulfilling with the armies one of those important missions, the difficulties of which his ardent patriotism always found the secret of surmounting.

CARNOT A MEMBER OF THE COMMITTEE OF PUBLIC SAFETY.

The concession which was required of me, if I conformed exactly to it, nevertheless authorises me to show myself less docile on the subject of another period of Carnot's life, which is still more stormy and difficult. Let us avoid—I willingly consent to it—carrying our attention back to certain irritating phases of our civil discords; for my own part, I will only put one condition on it; that is, that the memory of none of our members shall suffer by it. Well, Gentlemen, suppose for a moment that I be now silent concerning the "Member of the Committee of Public Safety;" would it not be concluded from my silence—nay more, would it not be right to conclude from thence,—that I have recognised the impossibility of repelling the violent, numerous, and *trenchant* attacks of which he was the object? These attacks Carnot, whilst living, was able to disdain; in me, on the contrary, it was incumbent to seek for their origin, and conscientiously weigh their value. I say it without exaggeration, no human power

should have decided me to cause the name of Carnot to re-echo here, unless I had discovered the honourable and patriotic causes of certain acts which the most atrocious of calumnies, political calumny, had soiled with its infected slaver. My work, furthermore, was not without some difficulties. Perhaps no one henceforth will have the opportunity to reunite its elements. In a few years, indeed, the colleagues and fellow labourers of Carnot, from whom I have been able to gather some lights and evidences, will have paid the debt of nature.

In 1793 the Convention was the only organised power in the State, capable of opposing an effective dyke against the overflow of enemies, who came from all parts of Europe to cast themselves on France, and menace her nationality. The nationality of a people is like honour: the slightest wound to it becomes mortal. Such were, Gentlemen, the sentiments of very many members of the Convention, whose memory France reveres; such were the ties which attached them to the perilous post whither election had called them.

In creating the "Committee of Public Safety" (6th April 1793), the Convention had reserved to itself the choice of its members. Up to the famous 31st of May, it counted only neutral members, or at any rate such as were strangers to the factions of the Assembly who were combating each other to the death. After several partial renewals it was composed, on the 11th September 1793, of Robespierre, Saint-Just, Couthon, Collot d'Herbois, Billaud-Varennes, Prieur (of the Marne), Prieur (of the Côte-d'Or), Carnot, Jean-bon Saint-André, Barère, Hérault de Séchelles, and Robert Lindet.

The Convention, when it delegated such great powers to the Committee of Public Safety, desired that every affair should be a subject of profound discussion and deliberation in that committee; that the majority of voices should decide. The decisions, to acquire the force of law, under pain of being null, must be furnished with a certain number of signatures. These prescriptions had the greatest of all faults, that of being completely impracticable. Man has discovered in our days the secret of going ten times as fast when he travels, of using less force when he acts, and of casting his searching gaze into the regions of infinity; but he has not yet discovered the means of reading a page of manuscript in less time than it formerly occupied. We must allow that in that respect, the most humble merchant's clerk would advance equally with Cæsar or Cicero, Descartes or Bossuet. The innumerable dispatches which the Committee of Public Safety received daily, from all points of the frontiers menaced or invaded, from all the towns and villages of the interior where the promoters of a

new political organization were in violent conflict with the prejudices and interests of the privileged classes, could not be maturely examined. Zeal, activity, and devotion were not sufficient to expedite so many weighty affairs; a reform was indispensable; it concerned the safety of France. Two different ways presented themselves: they could demand the re-organization of the Committee, or divide the work amongst its various members. The re-organization of the Committee, in presence of a powerful enemy and in the midst of unheard-of difficulties (such as no period of the history of nations had given an example of), would have excited in the Convention new ferments of disorder, enervated its magic power, and compromised the defence of the territory. The division of labour should prevail, and it did prevail. Carnot was charged with the organization of the armies and with their operations; Prieur (of the Côte-d'Or) with arming them; Robert Lindet with provisioning them; Robespierre, Saint-Just, Couthon, Billaud-Varennes, and Collot d'Herbois, reserved to themselves politics, general police, and measures of security. In each species of subject one signature alone was important, and carried responsibility; the others, though required by law, were to be regarded as the accomplishment of a simple formality: it was evident, indeed, that they would have to be given without discussion and even without examination.

Such were, Gentlemen, the bases of the agreement which Robert Lindet, for his personal security, caused to be put down in a written declaration, and by the aid of which the members of the Committee of Public Safety expected to be able, without passing beyond the terms of their mandate, to exorcise the storms which were menacing the country from all sides. This confiding arrangement will doubtless be blamed: some will cry out at its illegality, others at its imprudence. I will remind the first, that the members of the Committee, entangled in a faulty organization, were every day at issue with an impossibility, and that the word *impossible* is French, whatever national *amour propre* may have said of it at a period when the admirable triumphs of our armies seemed to warrant all hyperbolic speeches. The reproach of imprudence I admit without reserve. I add that, on the part of Carnot, this imprudence was voluntary; that in resigning himself to signing, without examination, the decisions of all his colleagues, he wittingly made the greatest of all sacrifices to France; that he placed his honour in the hands of several of his declared enemies; that, counting eventually on the tardy justice of posterity, he hoisted that almost superhuman motto of one of the most powerful organizations which the Revolution brought to land from the waves of the people,—that motto which moreover every

sincere patriot endowed with any warmth of soul might avow: *Perish my reputation sooner than my country.*

You will have already understood, Gentlemen, that my design is to divide into two distinct categories the members of the Committee of Public Safety, and the long series of its acts.

The terrible Committee contributed powerfully to the defence of the territory: thanks to the Committee! There was no other way of resisting the thousand passions let loose, than by vigour of determination; by energy of will; by seizing every where with a grasp of iron the barbarians who, auxiliaries of the foreigner, would have torn out the entrails of their country; the Committee showed itself energetic and vigorous; it often showed the grasp of iron: all praise to the Committee!

But, Gentlemen, firmness soon degenerates into frenzy; soon they immolate the rich for the sole reason that they are rich; soon terror reigns through France from one end to the other; terror carries mourning and despair without distinction, as well into the family of the common soldier as into that of the general; she seizes her victims equally in the humble dwelling of the artisan, as in the gilded palace of the former duke and peer; she spares neither age nor sex; she strikes blindly all shades of opinion; finally, adding dissimulation to cruelty, she parodies the forms of justice! Ah! Gentlemen, at this spectacle the heart grows faint, and hope withers; the liveliest and most ardent sympathies give place to profound grief.

I am aware that attempts have been made to explain, even to excuse, those bloody saturnalia, by referring them to the will of the people. But if I judge of the people of 1793, whom I have not known, by that which I saw in action in 1830, the explanation is false. I do not hesitate to say so. The people in a moment of effervescence and blindness, sometimes fall into culpable actions, but it has never associated itself with daily barbarities. It is degrading the people to say, that fear only could drive it to meet inimical hordes: nor are its sentiments better known, when it is insinuated that it wished for the death of one of the members of this Academy who honoured France by his genius; and the death of another of our co-academicians, who did honour to human nature by his virtue. No, Gentlemen; no! in the noble country of France, the death of Lavoisier, the death of Malesherbes, could not be ordered by considerations for the public good. No excuses for such crimes; they must be branded to-day, they must be branded to-morrow; they must be branded for ever. Devoted by sentiment, by conviction, by the irresistible power of logic to the worship of liberty, let us repel far from us the execrable thought, that the scaffold is the inevitable auxiliary of democracy.

The crimes that I have been openly denouncing have been in some measure personified by France, by Europe, by the whole world: these crimes are Robespierre! Some young, some estimable writers, who are now despoiling our revolutionary annals with the indefatigable patience of the Benedictines of former ages, think they have discovered that public opinion is quite wrong. According to them, Robespierre and his partisans have much less contributed to the sanguinary acts of terror, than the Billaud-Varennes, the Collot d'Herbois, or the Heberts. There is courage, Gentlemen, in coming forward as the defenders of a man, who for nearly half a century has been regarded as the symbol, the type, of political cruelty. On this claim alone could the new historians hope to be listened to without prejudice: an honourable character, joined to incontestable talent, gives them no less a right to the serious attention of the public. For my part, I have no business here to try to pierce those thick clouds; my subject does not require it: I will absolve Carnot from all participation in great crimes, without examining whether they should be imputed to Collot d'Herbois, or to Billaud-Varennes, rather than to Robespierre, Saint-Just, and Couthon.

In no instance of his long political career, was Carnot a party-man. Never was he found to try to bring forward his opinions, his systems, his principles, by tortuous ways that honour, that justice, that probity, could not have acknowledged.

In reporting on the 9th of June 1792, on the commission charged to propose some reparation in favour of the families of Theobald Dillon and of Berthois, who were massacred by their own troops before Lille, he does not coquette with his rigorous duty. Any other man, in such harassing times, might perhaps have thought it requisite to consider the susceptibility of the army; but he seemed to think no words too severe to brand such an odious act of wrong-headedness: he exclaimed, "I will not remind you of the circumstances of that atrocity. Posterity, in reading our history, will deem it rather the crime of a horde of cannibals, than that of a free people."

In 1792, some National Guards, under the name of *confederates*, assembled in great numbers at Soissons, and already formed there the nucleus of an army of reserve. All at once a report was spread at Paris, that the bread of those volunteers had been poisoned, that some monsters had mixed pounded glass with all the flour furnished to them, that two hundred soldiers had died already, and the hospitals were overflowing with sick men. The exasperation of the Parisian populace rose to its highest pitch: the depôt at Soissons was formed against the royal will; the crime then must be imputed to the King, to the Queen, to all

their adherents. Before acting, they only awaited the report of the commissary who had been sent to the camp. This commissary was Carnot. His truthful examination reduced all this fantasmagoria to nothing: there were no men dead: there were no men sick: the flour was not poisoned; but some panes of glass, broken by the wind, or by the ball of some recruit, had fallen from the window of an old church, and happened (not pulverised, but in large pieces,) to lie on one single bag of flour. The upright testimony of the honest man calmed the popular tempest.

He was not a *party-man* (understood of course in its unfavourable meaning): but one who, often charged with important missions to the armies and to the interior, fulfilled his duties with such moderation, that he could safely, when circumstances required it, without fear of being contradicted, publicly render to himself the testimony of never having caused the arrest of any one. By searching into the offices of the Committee of Safety, we should there find equally clear proofs of the benevolent indulgence of Carnot towards persons professing different political opinions from his, provided always that they were united to honest dealing, and a warm antipathy to the intervention of foreigners in the internal affairs of France. Thus we shall see, under the name of Michaux, amidst the fellow labourers of our academician, the celebrated Darçon, who had emigrated, but returned to his country. Still what occasion is there to drag our audience through individual instances, when a general reflection will lead to the same result? The Convention was the arena where the chiefs of the factions that divided the country, went to combat; yet it was in the *Clubs* that they created those adherents, and obtained that bodily strength, whose action, and even whose mere presence, often sufficed to annul the effects of the most eloquent discourses. If the Convention saw the thunder-cloud burst, it was outside its walls that it began to threaten, that it swelled, that it acquired an irresistible power. Men could not then acquire political influence without attending daily either at the Jacobins or at the Cordeliers, and mixing and taking part in all their debates: well, Gentlemen, Carnot did not belong to any of those associations; never did a word of his echo in those Clubs. In those troublous times, Carnot made himself exclusively *a man of the nation.*

The character was high, but not without danger. Robespierre especially was jealous of him, and exclaimed in one of his harangues: "To have taken the command of all the military operations, is decidedly *an act of egotism;* obstinately refusing to take any part in the affairs of internal police, is contriving means of accommodation with the enemies of the country."—He said to Cambon on another occasion: "I am in despair at not com-

prehending anything of the intersection of lines and of tints, that I see on those maps. Ah! if I had studied the military art in my youth, I should not now be obliged, whenever our armies are treated of, to submit to the supremacy of the odious Carnot." This animosity began from the epoch when our fellow academician blamed the *coup d'état* (as such) under which the Gironde fell. About the same time, Saint-Just accused him of *moderatism,* and demanded that he should be tried for having refused, while with the army of the North, to put his signature to the order for arresting General O'Moran. Carnot always came out safe and sound from these terrible crises; not from a sentiment of justice or affection, but because every one, friend as well as foe, felt the impossibility of replacing him effectually in his special military character by any other *conventionalist!*

Similar relations between the co-members of a council would now appear fabulous! Is it *my* fault then, if our weak patriotism cannot conceive all the extent of the sacrifices that our fathers imposed on themselves to save our country?

You will remember, Gentlemen, that I did not hesitate to place in the first rank of these sacrifices the obligation which our colleague felt, of blindly signing a quantity of decrees issued by his colleagues. I have explained how this necessity had *manifested* itself; well, it was so abused, that on one occasion, Carnot was made to sign the order for arresting his own secretary; another time, that for arresting the *restaurateur* in whose house he took his meals. The word *infernal* seems to me really too feeble for characterising such acts; and yet, to the honour of our colleague, we must almost congratulate ourselves that they occurred, since they yield an insurmountable and speaking proof of the written arrangement which was agreed to in committee, in the name of *the safety of the country.*

I had read, even in royalist works, and I had read also in some writings published by republicans, that Carnot had saved, in the Committee of Public Safety, more men than his colleagues had immolated. Carnot, then, did not absent himself from the meetings except when military affairs entirely absorbed his time; Carnot then, sometimes attended the deliberations of the Committee, and on those occasions innocence could depend on an advocate full of feeling and firmness. Only a few days ago, chance enabled me to discover that the part of volunteer defender was not the only one that Carnot took upon himself.

There is amongst you, Gentlemen, a venerable academician equally versed in theoretical and in applied mathematics; he has gloriously attached his name to some useful labours, and to some vast projects that the future, perhaps, will realise. He has gone

through a long career, without making, certainly without deserving, an enemy! and yet his head was once menaced, and some wretches wished to make it fall, at the very time that he was projecting one of the scientific monuments that have reflected most honour on the revolutionary æra. An anonymous letter informed our colleague of his danger. The storm is dissipated, but it may gather again in an instant; the friendly hand traces out a line of conduct; rules of prudence point out the necessity of preparing a retreat. Nor will it leave the work unfinished, but will again take up the pen if the danger re-appears.

The anonymous writer, Gentlemen, was Carnot; the geometer whom he thus preserved to science, and to our affections, was M. de Prony. At that epoch Prony and Carnot had never seen each other.

The years 1793 and 1794 were characterised by two sorts of *terror:* the terror of the interior, I have just proved, Gentlemen, our colleague was always a stranger to, as to anything criminal in it; but the terror which the French soldiers inspired in the innumerable enemies that came from every part of Europe to assail our frontiers,—this sort of terror was indeed the work of Carnot; it was glorious; the recollection of it will be immortal; I claim it for the memory of our colleague; I claim it also for the honour of the Academy. You will not refuse, Gentlemen, again to follow Carnot in this fine and brilliant phase of his public career. I am assured in this hope by your devotion to our country.

CARNOT ENTRUSTED WITH THE ORGANIZATION AND DIRECTION OF OUR ARMIES.

At divers epochs, in France as well as in other countries, simple administrators have been seen successfully to occupy the eminent positions of Minister of War, and Minister of Marine. The General-in-chief, the Admiral, was then entrusted with a command, with *carte blanche* as to the operations, and the ministers had nothing farther to do than to send regular and opportune provisions and reinforcements. Would you believe it, Gentlemen? it was in this confined circle that bad faith and envy wished to confine the decisive influence that Carnot exercised on our destinies. But it will be easy for us in a few words to tear to pieces this web of hideous ingratitude.

When our colleague became, in August 1793, member of the Committee of Public Safety, France was passing through a frightful crisis. The wreck of Dumouriez's army was repulsed from one position to another; Valenciennes, Condé, opened their gates to the enemy; Mayence, pressed by famine, and without the hope of relief, capitulated; two Spanish armies invaded our territory;

20,000 Piedmontese were crossing the Alps; the 40,000 Vendéans of Cathelineau were taking Bressuire, Thouars, Saumur, Angers; they menaced Tours, le Mans, and attacked Nantes on the right bank of the Loire, whilst Charette manœuvred on the opposite bank; Toulon received an English fleet into its port; in a word, our principal cities, Marseilles, Caen, Lyons, separated themselves violently from the central government.

You have now before your eyes, Gentlemen, a faint image of the dangers which menaced our country; and have some people dared to pretend that the Convention, that the terrible Convention, hoped to escape from the imminent catastrophe that almost all Europe thought inevitable, without even establishing a certain connection in the operations of its generals? and can it have been imagined that, in entrusting one of its members with the almost sovereign direction of its military affairs, it expected from him only the methodical measures and regulations compassed by a purveyor or *intendant* of an army? No, no! no one could possibly in good faith adopt such ideas.

Do not, however, believe that I undervalue Carnot's administrative services. I admire, on the contrary, their noble simplicity. There was not, assuredly, at that time in his administration, either that inextricable series of scribbling which the smallest affair entails on us in the present day; nor that artistic network entangling every one, from the junior clerk of the office up to the head of the department, in so intricate a manner, that the firmest and boldest hand could not hope to break a link or separate the elements. The chief used then to take a responsible, personal, and direct cognizance of the dispatches that were addressed to him; the conceptions of the chosen man were not then exposed to perish under the blows of an envious multitude of poor intellects; a mere serjeant of infantry, then, (young Hoche) did not work only on the dusty papers in the archives, when he composed *A Memoir on the Means of penetrating into Belgium;* then, the perusal of this work drew from Carnot this prophetic exclamation: "That is a serjeant of infantry who will make his way." Then this serjeant, watched by the eye in all his actions, became, in the space of a few months, captain, colonel, brigadier-general, general of division, and general in chief; it was not then only a small class that was invested with the privilege of furnishing the chiefs of our armies; then, both in fact and by right, each soldier had promotions in his cartridge-box: splendid actions brought them out; yet the military force then, notwithstanding the important services that it rendered to the country, notwithstanding the disorders of that epoch, respectfully lowered its fasces before the civil authority, the proxy of the nation.

Let us cast a glance towards another phase of the military administration, and Carnot will not appear to us either less great or less successful.

There was a want of pure copper; at the cry of the distressed nation, science discovered in the bells of the convents, of the churches, of the public clocks, an inexhaustible mine, whence she might extract, without delay, all the metal that England, Sweden, and Russia refused her. There was no saltpetre; some lands, where formerly only enough of this substance would have been sought to add certainty to some delicate chemical analysis, furnish enough for all the requirements of our armies and our fleets. The preparation of shoe-leather used to require whole months of labour; such long delays did not suit the wants of our soldiers, and consequently the tanner's art received unhoped-for improvements; days will now take the place of months. The manufacture of arms is so minute, that its slowness seems inevitable; but immediately some mechanical aids are brought into action to strengthen, to direct, to take the place of the workman; the products increase in proportion to the demand for them. Until the year 1794, balloons had been only a mere object of curiosity; but at the battle of Fleurus, a balloon carries General Morlot into the region of the clouds; from thence the smallest manœuvres of the enemy are perceived, are signalised instantly, and this invention of purely French origin procures a splendid triumph for our army. Graphite pencils (black lead) are the pen and ink of an officer on a campaign; it is with the pencil that he writes a few words on the pummel of his saddle, which send thousands of infantry, of cavalry, of artillery-men into the thickest of the fight; graphite is one of the substances that Nature seems to have denied to our soil; the Committee of Public Safety orders it to be created of all sizes, and this order to make a discovery is executed without delay; thus the country is enriched with a new branch of industry. To be brief, for I must resign myself to not saying everything, the first ideas of the telegraph are drawn from some folio books, wherein they had lain useless for ages; they are perfected, they are extended, they are applied, and from that moment the armies receive their orders in a few minutes; the Committee of Public Safety in Paris is enabled to follow all the events of the war, to the east, the north, the west, as if it were seated in the midst of the combatants.

These somewhat spontaneous creations, these patriotic directions given to so many noble intellects, this art, now lost, of exciting genius, of dragging it from its habitual indolence, will always occupy a large place in the annals of the Committee of Public Safety, and in the history of our colleague's life. With-

out departing, moreover, from the subject that now occupies us we might still register many other services.

Carnot was one of that very small number of men who, in 1793 firmly believed that the Republic would sooner or later triumpl over its innumerable enemies. Thus, although he gave to th present as large a portion of attention as circumstances demanded yet having an eye to the future in his administration, he enriche France with many great institutions, the happy effects of whicl can only be slowly developed.

If time allowed me, I should have to cite amongst the grea establishments towards the formation of which Carnot contributed the first Normal School, the Polytechnic School, the Museum o Natural History, the Conservatory of Arts and Trades; an amongst the labours that he encouraged by his suffrage, was th measurement of the earth, the establishment of the new system o weights and measures, and the great, the incomparable statisti tables.

These are noble titles, Gentlemen, for an æra of destruction.

The Convention put into the hands of Carnot the colossal, bu incoherent mass of the *requisition.* It was requisite to organize i to discipline it, to instruct it: Carnot produced from it *fourtee armies.* It was also requisite to create for it some able chiefs our colleague knew, with a certain Athenian general, that *a army of Deer commanded by a Lion, would be worth more than a army of Lions commanded by a Deer.* Carnot dug without inter mission in the fruitful and inexhaustible mine of junior officers as I before said, his penetrating eye sought in the most obscur ranks for talent united with courage, with disinterestedness, an elevated it rapidly to the highest grades. It was necessary to co ordinate so many various movements! Carnot, like Atlas in th fable, carried alone, during several years, the weight of all th military events in Europe; he wrote with his own hand to th generals; he gave them detailed orders, wherein all the eventu alities were minutely foreseen; his plans, the one that he addresse to Pichegru for instance, on the 21 Ventose, year II, seeme the result of real *divination.* Facts occurred so entirely just fying the forethought of our colleague, that to write an accour of the memorable campaign of 1794, there would be scarcely few proper names of villages to be altered in the instructions th he addressed to the commander-in-chief. The places where attack were to be made, those where they were to limit themselves t demonstrations, to skirmishes; the strength of each garrison, each post, all is indicated, all is regulated with admirable pr cision. It was by orders from Carnot that Hoche one day di appeared from before the Prussian army, traversed the Vosge

and, uniting himself to the army of the Rhine, went to strike a decisive blow on Wurmsur, which occasioned the deliverance of Alsace.

In 1793, while the enemy was expecting, according to the classic principles of strategy, to see our troops advance from the Moselle to the Rhine; whilst he was accumulating formidable means of resistance on the latter river, Carnot, without troubling himself about old theories, detached unawares 40,000 men from the army of the Moselle, and sent them to the Meuse by forced marches. Such was the celebrated manœuvre which decided the success of the campaign of 1793, during which the Austrian and Dutch generals had the double chagrin of being constantly beaten, and this against all rules. Yes, Gentlemen, the national tribune was but just, on the day when it echoed these noble words, which have now become historical: "*Carnot has organized victory.*"

CARNOT ON THE FIELD OF BATTLE AT WATTIGNIES.

It might be said of the French armies, as of certain painters, that they have had *various styles.* On the day of battle, it is true, the imperial armies and the republican armies precipitated themselves on the enemy with the same intrepidity; with this exception all else was different. The imperial soldier saw his country only in the ARMY; it was for the honour, for the glory of the army, that he shed his blood at Wagram, at Sommo-Sierra, at the Moscowa. The republican soldier fought for his COUNTRY: the national independence was the thought that, above all others, animated him in the combat; as to recompenses, he did not even dream of them.

Follow those same soldiers into private life, and you will see this dissimilitude continue. The imperialist remains a soldier both in his sentiments and in his manners; the republican, confounded in the mass of the population, becomes soon undistinguishable from an artisan, from a labourer, who had never quitted his workshop or his plough.

It is these shades, cleverly seized, artistically reproduced, that from the first day struck the public with such a lively admiration for the productions of David. One day, an officer of the Empire, known for his brilliant valour, said to me in the library of the Institute: "I cannot reconcile myself to seeing General Carnot in a man drest *in short breeches and blue stockings.*" I took the opposite view; upon which he added, "Well, be it so! blue stockings may suit a general who was never *baptized by fire!*" Yesterday, also, with less roughness it is true, in word, one of our co-academicians reproduced in my presence the same thought. I

shall then fulfil a duty by proving that, when occasion required, the man in blue stockings knew well how to risk his life.

The Prince of Cobourg, at the head of sixty thousand men, occupied all the outlets of the forest of Mormale, and blockaded Maubeuge. This town once taken, the Austrians would have met with no more serious obstacles to their reaching Paris. Carnot perceives the danger; he persuades his colleagues in the Committee of Public Safety that our army, notwithstanding its numerical inferiority, can give battle; that it must attack the enemy in its apparently impregnable positions. It was one of those critical moments that decide the fate, the existence of nations. General Jourdan hesitates under such a terrible responsibility. Carnot goes to the army; in a few hours all is arranged, all is agreed upon; the troops open out, they fall upon their enemies; but the latter are so numerous, they occupy so well chosen a position, they have dug so many entrenchments, they have furnished them so formidably with artillery, that success is uncertain. At the close of the day, our right wing had gained some ground; but the left wing had perhaps lost more. It had moreover left some guns in possession of the Austrians. Let us strengthen the left wing, exclaimed the old tacticians. No, no, replied Carnot; what signifies by which wing we triumph. It was necessary, with good will or ill will, to yield to the authority of the people's representative! The night is employed in breaking up the wing already compromised; its principal troops are marched to the right, and when the sun rose, it was in some measure a new army that Cobourg found opposed to him. The battle recommenced with fresh fury. Shut up in their redoubts, protected by woods, by palisades, by quickset hedges, the Austrians resist valiantly; one of our attacking columns is repulsed, and begins to disperse! Oh! who could describe the cruel anguish that Carnot experienced. Doubtless his imagination already represents to him the enemy penetrating into the capital, defiling along the boulevards, and abandoning themselves to those acts of Vandalism, with which in so many proclamations, in so many insolent manifestoes, we had been threatened! These distracting thoughts, however, do not abate his courage: Carnot rallies his soldiers, re-forms them on a plot of ground; solemnly, before the whole army, degrades the general who, in disobeying positive orders, had allowed himself to be defeated; seizes the musket of a grenadier, and marches at the head of the column, in the civil costume of the representative of the nation. Nothing could now withstand the impetuosity of our troops; the charges of the Austrian cavalry are repelled with the bayonet; all who enter into the excavated roads around Wattignies are sure to meet with death. Carnot finally pene-

trated into the village, the very key of the position of the enemy's army, over heaps of dead bodies, and from that moment the siege of Maubeuge was at an end.

It will be asked, no doubt, where Carnot had gained this firmness, this vigour, the military *coup d'œil,* that knowledge of troops? Seek not for the source but in his ardent patriotism. It was at Wattignies that for the first time he heard the musketry and cannon of the enemy. But I am mistaken, Gentlemen; it is the second, and not the first time: the first time, Carnot, marching as at Wattignies, musket in hand, at the head of a new levy of soldiers, carried the town of Furnez by assault, then occupied by the English.

The battle of Wattignies, considered as to its results, will always occupy one of the foremost places in the annals of the French Revolution. I should probably be less positive on the difficulties of that day, compared with so many others, if I could not support myself by the opinion of the Prince of Cobourg himself. When he saw the French battalions begin to break, that general could not find terms too strong to express, in presence of his staff, the confidence that he felt in the number and ardour of his troops, and in the obstacles of all sorts, both natural and artificial, that the uneven ground occupied by the Austrians presented to the assailants. He exclaimed: "The Republicans are excellent soldiers; but if they dislodge me from this position, I will consent to become a Republican myself." Certainly nothing more decided or more energetic could issue from the mouth of Cobourg. For my part, I could not conceive a more glorious bulletin of the battle of Wattignies!

The German author from whom I have borrowed this anecdote does not say whether, after having dislodged him, the French summoned the Austrian general to keep his word. I have some reason to suppose that, notwithstanding their spirit of propaganda, they disdained a recruit who would have submitted, but whose vocation seemed very uncertain.

STATISTICS OF THE OPERATIONS OF THE ARMIES.

Carnot felt the propriety, the want, of showing towards the national armies a deference from which absolute governments formerly felt themselves free, whilst their soldiers were enrolled at a money price: each year he had to unroll to the eyes of the nation a detailed table of the battles given by our legions, and of the effects that had thence resulted. Here follows the conclusion of the recital of the campaign of seventeen months, during which

the troops of the Republic never laid down their arms for a single day.

Twenty-seven victories, eight of which were pitched battles; 120 combats of minor importance; 80,000 enemies killed; 91,000 prisoners; 116 fortresses or strong towns taken, 36 of which had required to be besieged or blockaded; 230 forts or redoubts taken; 3,800 guns of various sizes; 70,000 muskets; 1,900 *milliers* (*tons*) of gunpowder; 90 flags. Let people, if they dare, after reading this table, say that statistics are not eloquent!

CARNOT, NAMED BY FOURTEEN DEPARTMENTS, ENTERS THE COUNCIL OF THE ELDERS, AND THEN THE EXECUTIVE DIRECTORY. — HOCHE SENT TO LA VENDÉE, MOREAU AND JOURDAN TO THE RHINE, AND BONAPARTE TO ITALY.

Carnot quitted the Council of Public Safety shortly before the insurrection of the Parisian sections against the Convention. Carry back your recollection towards the military events that followed the forced, though legal retreat of our colleague, and you will see almost everywhere that victory abandoned the standards of the Republic, and reverses succeeded each other, as triumphs did before; all the springs were unbent, mistrust and discouragement took possession of every mind; and you will then understand, better than by an uninterrupted series of brilliant successes, of what importance the genius of one man alone may be to the destiny of nations.

Carnot was called to the legislature which succeeded to the National Convention by *fourteen departments.* If I were allowed to express a personal sentiment, I would say how pleased I have been to find the name of the department of the Eastern Pyrenees, in the list of those which tried to reward our great citizen for the outrages that a handful of members, excited by the butcher Legendre, cast upon him on several occasions. A short time after he entered the Council of the Elders, Carnot, on the refusal of Sieyès, became one of the five members of the Executive Directory.

At the moment when he for the second time was thus called to direct our armies, the Republic had reached the verge of an abyss. The public treasury was empty. The Directory had great trouble even in procuring clerks and servants in their office, so much was it thought to be insolvent. The despatching of a courier was often delayed on account of the impossibility of providing for the expenses of the journey; the generals themselves no longer received the *eight francs* (I AM NOT MISTAKEN), the *eight francs per month* "en numéraire" (in cash), that had been granted to

them, as a supplement to their pay in assignats; the agricultural producers no longer supplied the markets; the manufacturers refused to sell their products, because there was a right to pay for them in paper-money, and paper-money then was of no value. From one end of France to the other, famine had thrown people into an extreme state of irritation, which daily manifested itself in sanguinary disorders. The army offered a no less deplorable aspect: it was deficient in means of transport, in clothing, in shoes, in munitions. Misery had engendered a want of discipline. Pichegru was weaving criminal relations with the Prince of Condé, allowed himself to be beaten at Heidelberg, compromised the army of Jourdan, evacuated Mannheim, raised the siege of Mayence, and ceded the frontier of the Rhine to the Austrians. War recommenced in La Vendée; the English threatened us with a descent in the Pays-Bas, and on our own coasts. In a word, on our Alpine frontier, Scherer and Kellermann painfully sustained a defensive war against the united forces of the Emperor of Austria, the King of Sardinia, and the confederated Italian princes.

Gentlemen, great strength of mind, united to the most ardent patriotism, was requisite, under such cruel circumstances, to induce men to accept the burden of public affairs. Let us add that Carnot was so little blind to the faults of the Constitution of the year III., and, above all, to the inconveniences of a multiple executive power, that he had publicly pointed them out in the midst of the Convention, at the time when this constitution was discussed. He then exclaimed: "The destinies of the state will henceforward depend only on the personal character of five men. The more these characters differ, the more dissimilar will be the views of these five directors, and the more will the state have to suffer from their alternate influence." The majority disdained these just apprehensions; faithful to a line of conduct from which he was never seen to swerve, Carnot submitted without a murmur; and, as soon as the new government had received the legal sanction, he served it with the same energy, zeal, and devotion that he had before displayed as a member of the Committee of Public Safety.

La Vendée was on fire; Hoche receives orders from Carnot to pacify it, together with a new system of operations. This republican general complies, triumphs over Charette, takes possession of Stofflet, and clears the Morbihan of the numerous bands of *chouans* who ravaged it. In less than eight months, the civil war, that impious war, in which, however, great courage was displayed on both sides, ceased to desolate our territory.

On the Rhine, our armies are placed under the command of Jourdan and Moreau. A scientific and profound plan of the cam-

paign connected the movements of those two generals, and soo carried their victorious troops into the heart of Germany.

In La Vendée, in Germany, on the Rhine, Carnot, as we hav shown, had infused confidence into officers already celebrate by memorable triumphs. The command of the army of Italy h gave, on the contrary, to a general only twenty-five years of age whose known claims were then restricted to some secondar services that he achieved during the siege of Toulon, and to th easy defeat of the Parisian Sectionaries, on the 13th Vendémiaire year III., on the humble fields of battle of the Pont Royal an the Rue St.-Honoré, and the steps of St.-Roch. I here claim fo Carnot the honour of having personally pointed out and selected the young General Bonaparte for the command of our third army, because it legitimately belongs to him; because this choice was long unjustly considered as the result of a boudoir intrigue; and because every one, I think, must be glad to see the history o the incomparable campaign of Italy purified from such a stain. I have thought, in short, that I ought not to neglect to show you your colleague discerning with infinite perspicacity the hero o Rivoli, of Arcole, of Castiglione, through the bark of timidity, o reserve,— let us out with the true word, of awkwardness,— that everybody then remarked in the *protégé* of Barras.

I foresee all the incredulity I should meet with, if I were to venture on still farther extending the limits of the influence that our colleague exercised over the Italian campaign; and yet, should I not have found, even in the small number of official documents already known to the public, under date of the 10th Floréal, year IV., for example, a despatch from the head-quarters of Chérasco, in which Bonaparte writes to Carnot: — "The armistice concluded between the King of Sardinia and ourselves, enables me to communicate through Turin, that is to say, to spare half the journey; I *could therefore quickly receive* YOUR ORDERS AND LEARN YOUR INTENTIONS, AS TO THE DIRECTION TO BE GIVEN TO THE ARMY." A letter to the Minister of Finance, of the 2nd Prairial, year IV., dated from head-quarters at Milan, would afford the following sentence: — "The Executive Directory, who named me to the command of this army, HAS ARRANGED A PLAN OF OFFENSIVE WARFARE which requires prompt measures and extraordinary resources."

The 2nd of Prairial, year IV., (May 21. 1796) Carnot wrote thus to the young general: — "Attack Beaulieu before reinforcements can reach him; do not neglect anything to prevent this junction; you must not weaken yourself before him, and above all, do not, by disastrously dividing your force, give him the means of beating us in detail, and retaking the ground he has lost. After

the defeat of Beaulieu, you will make the expedition to Leghorn. . . . . The intention of the Directory is, that the army shall not pass beyond the Tyrol, until after the expedition to the south of Italy."

Doubtless, these general orders are not the campaign of Italy. No human intelligence could foresee either the route that General Beaulieu would follow after his separation from the Piedmontese army, nor the manœuvres of Wurmser, nor the long resistance made at Mantua by that old general, nor the marches of Alvinzi, nor many glorious incidents which I abstain from recalling; without doubt it required all the hardihood and genius of Bonaparte, and the co-operation of such officers as Masséna, Augereau, Lannes, Murat, Rampon, to annihilate in a few months three large Austrian armies. Finally, all that I have wished to say is, that it would be unjust to entirely omit the name of Carnot in reciting those immortal campaigns.

I should have a right to say even more were we studying another phase of those wars,—their moral and civilising phase. Who does not remember those treaties of peace, in which masterpieces of painting and of sculpture were inducements to pardon perfidy and treachery in our enemies, and the official visits of our victorious generals to diffident learned men, rendered illustrious by important discoveries? Well, Gentlemen, all this, whatever people may say of it, was prescribed by Carnot. Will any doubts still be entertained if I transcribe the following letter from our colleague, dated

"24th of Prairial, year IV.

"General, in recommending you, by our letter of the 26th Floréal, to visit and receive the celebrated artists of the countries in which you happen to be, we have especially designated the great astronomer Oriani, of Milan, as deserving of being protected and honoured by the republican troops. The Directory will learn with satisfaction that you have fulfilled its intentions respecting this learned and distinguished man, and it requests you consequently to relate what you have done to prove to citizen Oriani the interest and the esteem that the French have always felt for him, and to testify that they know how to unite the love of glory and liberty with a love for the arts and for talent."

PUBLICATION OF THE WORK ENTITLED "REFLECTIONS ON THE METAPHYSICS OF THE INFINITESIMAL CALCULUS."

The word science, which the series of events has just brought to my pen, reminds me that this epoch is that of the publication of one of Carnot's mathematical works. I am aware how fatiguing it will be to you to listen to the analysis of it; but it is quite

necessary that the *savant* also should be occasionally represented in this assembly. The early and very remarkable work on machines of which we gave an idea, has sufficiently indicated how much we may expect from the firm, lucid, and penetrating mind of Carnot. It was then a brilliant and glorious future which the young officer brought as an offering to his country, when, obeying the voice of his fellow-citizens, he exchanged the smooth, tranquil life of the mathematician, for the adventurous and rock-bestrewn career of the tribune. This sacrifice, moreover, he did not make without regret; for geometry was always his favourite relaxation. Debarred by imperious daily duties from the pleasure of "*measuring himself with*" the grand problems whose solution requires years of continuous and persevering effort, Carnot chose those difficult but circumscribed questions which may be taken up, abandoned, and taken up again, by fits and starts; which an elevated mind capable of coping with difficult subjects, developes and fathoms without paper or pencil, either during a walk, in the midst of the excitements of the crowd, the gaieties of a banquet, or the vigils of laborious nights; in a word, he directed his meditations towards the "*metaphysics of the calculus.*" In the present day such researches would be, I fear, but little relished; nevertheless, if we recur to the times when mathematical studies gradually led to the consideration of quantities of such different natures, we shall be amply aware of the apprehension with which they inspired exact philosophers, and must acknowledge that, on many points, it is rather habit than true science which has rendered us more confident.

Amongst the quantities to which I have alluded, the "*irrational*" presented themselves first. The ancients scrupulously avoided using them; the moderns would also have wished to avoid the use of them; "*but they*" (the quantities) "*gained the day by their numbers,*" says the ingenious author of the "*Geometry of Infinites.*"

To the quantities which were not numerically assignable, succeeded the impossible quantities, the "*imaginary quantities,*" regular symbols of which it would be vain to attempt to give, not only the exact values, but even mere approximations. These imaginaries are nevertheless used in combination by addition and subtraction; they are multiplied and divided, the one by the other in the same manner as real quantities; at the end of the calculation the imaginaries sometimes disappear amongst the transformations which they undergo, and the result is then held to be quite as certain as if it had been arrived at without the help of these algebraic hieroglyphics. It must be confessed that, though thousands on thousands of applications of the calculus justify this confidence, few geometers fail to take credit to themselves for the

absence of imaginary quantities in the demonstrations where they have been able to avoid them.

The "*infinite*" first made its irruption into geometry on the day when Archimedes determined the approximate proportion of the diameter to the circumference by assimilating the circle to a polygon "*with an infinite number of sides.*" Bonaventura Cavalieri afterwards went much farther in the same field of research; various considerations led him to distinguish some "*infinitely great* quantities" of several orders, from some *infinite quantities* which were nevertheless *infinitely smaller* than other quantities. Can we be astonished that, at sight of such results, and notwithstanding his lively predilection for combinations, which had led him to veritable discoveries, the ingenious Italian author should have exclaimed, in the style of that period, "*Here are difficulties of which even the arms of Achilles could make nothing!*"

The "*infinitely small*" quantities had, for their part, slipped into geometry even before the "*infinitely great,*" and this not only to facilitate or abridge such and such demonstrations, but as the immediate and necessary result of certain elementary properties of curves.

Let us examine, in effect, the properties of the most simple of all — the circumference of a circle; and by that we will not understand the rugged clumsy curve which we should succeed in drawing by the aid of our compasses or best geometrical drawing-pens; but really the circumference of a circle endowed with an ideal perfection, really a curve without thickness and without roughness of any sort. Let us, in imagination, draw a tangent to this curve. At the point where the tangent and the curve touch one another, they will form an angle, which has been called the "*angle of contact.*" This angle, since the first origin of mathematical science, has been the object of the most serious reflections of geometers. Since two thousand years ago it has been rigorously demonstrated that no straight line, drawn from the apex of the angle of contact, can be included between its two sides, and that it cannot pass between the curve and the tangent. Well, I ask, what else is that angle into which an infinitely fine straight line cannot be introduced or insinuated, but an infinitely small quantity?

The infinitely small angle of contact, into which no straight line can be introduced, may nevertheless include between its two sides millions of circumferences of circles, all greater than the first. This truth is established by reasoning of an incontestable and uncontested force. Here, then, we have, in the very heart of elementary geometry, an infinitely small quantity, and, what is still more incomprehensible, susceptible of being divided as much

as we please! The human intellect was humiliated and lost in face of such results; but, at any rate, these were results, and it submitted.

The infinitely small quantities which Leibnitz introduced into his differential calculus excited more scruples. This great geometer distinguished several orders of them: those of the second order might be neglected in relation with the infinitely small of the first: these infinitely small of the first order in their turn disappeared before finite quantities. At each transformation of the formulæ it might be possible, according to this hierarchy, to disembarrass one's self of fresh quantities; and, nevertheless, one was obliged to believe, to admit, that the definitive results were rigorously exact; that the infinitesimal calculus was not merely a mere method of approximation. Such was, considering the whole thing, the origin of the strong and tenacious opposition which the new calculus raised up at its birth; such was also the difficulty which a man equally celebrated as a geometer and a theologian, Berkeley, bishop of Cloyne, had in view when he exclaimed, addressing himself to the incredulous in matters of religion, "Look at the science of mathematics: does it not admit mysteries more incomprehensible than those of religion?"

These mysteries, at the present day, exist no longer for those who desire to become initiated in the knowledge of the methods which constitute the differential calculus in Newton's theory of fluxions; in a paper wherein D'Alembert introduces the consideration of the limits towards which the ratios of the finite differences of functions converge; or, indeed, in Lagrange's *Theory of Analytical Functions.* Nevertheless, Leibnitz's course has prevailed, because it is more simple, easier to recollect, and more convenient in practice. It is, then, important to study it in itself, to penetrate into its essence, and to assure one's self of the perfect exactness of the rules which it furnishes, without the necessity of corroborating them by the results of the calculus of fluxions, or of limits, or of functions. That task, — I mean the search for the true spirt of differential analysis,—forms the principal object of the book which Carnot published, in 1799, under the modest title of *Reflections on the Metaphysics of the Infinitesimal Calculus.* I am bold enough to assert that the authors, otherwise so excellent, of the best treatises on the differential calculus, have not sufficiently consulted the work of our colleague. The advantages which ought to result from the immediate introduction of infinitely small or elementary quantities into formulas; the considerations by help of which it may be proved that the calculator, by afterwards throwing aside these quantities, will arrive nevertheless at mathematically exact results, by means of certain com-

pensations for errors; in a word, the fundamental and characteristic traits of Leibnitz's method, are analysed by Carnot with a clearness, a certainty of judgment, and an ingenuity, which we should look for in vain elsewhere, though the question has been the object of the reflections and researches of the greatest geometers of Europe.

CARNOT BEING "FRUCTIDORISÉ" IS OBLIGED TO RECUR TO FLIGHT.—HE IS ERASED FROM THE LIST OF THE INSTITUTE, AND SUCCEEDED BY GENERAL BONAPARTE.

France has always shown itself an idolator of military glory. Satisfy this passion largely in a national war, and you need not be uneasy about the administration of the interior, however imperfect it may be. The sympathies of the people, and in case of need even their entire submission, may be gained by any government that takes care to adorn itself monthly with a new victory over its external enemies. I perceive but one exception to this rule in our annals. It is also requisite, however, that, by an assimilation, too often deceitful, the legal representatives of the country should be considered as the faithful interpreters of the wishes, the sentiments, the opinions of the majority. The exception to which I am about to allude, will be furnished by our Directorial government.

When the elections of the year V. brought a reinforcement of royalists to the two minorities of the Council of Five-Hundred, and of the Elders, who till then had limited themselves to making a very moderate opposition to the Directory; when, strong in what they thought the popular support, the minority, fancying that they had become the majority, took off the mask so far as to name for the presidency of the Council of Five-Hundred that same Pichegru, who not long before had branded with treason the laurels that he had gained in Holland in the name of the Republic; when the enemies of the Directorial power openly unveiled their projects in the saloons of the celebrated Clichy Club; when the recriminations, the reciprocal accusations, that had reached the utmost violence, were already succeeded by deeds of violence against patriots, and the gainers of national property,—our troops were yet everywhere triumphant. The army of the Rhine and Moselle under the orders of Moreau, the army of the Sambre and Meuse, commanded by Jourdan, had gloriously crossed the Rhine; they were marching into the heart of Germany; the army of Italy was only twenty leagues from Vienna; at Leoben, Bonaparte signed the preliminaries of the much wished-for treaty of peace. Without compro-

mising the negotiations, he could show himself touchy about mere questions of etiquette; he could BLUNTLY refuse to let the name of the Emperor of Germany precede that of the French Republic in the protocols; he could also, when General Meerwald, and the Marquis del Gallo talked to him about gratitude, answer, without a boast, in the following memorable words: "The French Republic does not require to be recognised; it is in Europe what the sun is on the horizon: so much the worse for those who will not see and profit by it." Is it then surprising, Gentlemen, I ask you, that in so favourable a position of our foreign affairs, Carnot believed in the possibility of a conciliation between the political parties into which the country was divided; that he refused (I purposely use his own words) to exorcise danger by going beyond the limits of the Constitution; that he firmly repelled any thought of a *coup-d'état*, — a very convenient way assuredly of getting out of a scrape; but a dangerous way, and one that almost always ends by becoming injurious to the very persons who expected to benefit by it?

I should have much wished, Gentlemen, to have entered more deeply into an examination of the part that Carnot acted at that critical epoch of the Revolution. I have not neglected anything to raise at least a corner of the veil which still covers an event that so greatly influenced the fate of our colleague, and that of the country: but my efforts, I acknowledge, have been unsuccessful. Documents are not wanting, but they almost all emanate from writers too much interested, either in excusing or in branding the 18th of Fructidor, not to be suspected. The recriminations full of bitterness, of violence against each other, to which some old colleagues then abandoned themselves, have reminded me of that wise declaration by Montesquieu: "Do not listen either to Father Tournemine or to me, when we are speaking of each other, for we have ceased to be friends." The antecedents, the opinions, the character, the known and avowed actions of the various persons who caused the *coup-d'état*, — or became the victims of it, would not have been any better guide. I should have seen Hoche march at one moment against his constant and zealous protector, against him who had saved his life under the rule of Robespierre, and who, in 1793, transformed the trimmings of the young sergeant into the epaulettes of a full general. I should have found Bonaparte contributing by his delegate Augereau, to the upsetting, and to the proscription of the only Director with whom he had continued intimately connected during the campaign of Italy. I should have seen him on his journey to Geneva have the banker Bontemps arrested, under pretext that he had favoured the escape of that

same Carnot to whom a few months before, he, Bonaparte, wrote from Plaisance (20th of Floréal, year IV.), from Milan (the 20th of Prairial, the same year), from Verona (the 9th of Pluviose year V.): "I owe you special thanks for the attention that you kindly show to my wife; I recommend her to you; she is a sincere patriot, and I love her to madness . . . . I will deserve your esteem; I beg of you to continue your friendship for me. . . . . The sweetest recompense for the fatigues, the dangers, the chances of this profession, is the approbation of the small number of men whom we appreciate. . . . . . I have always had to rejoice in the marks of friendship that you have shown to me and mine, and I shall always be truly grateful to you for them. . . . . The esteem of a small number of persons like yourself, that of my brother officers, of the soldier, interest me deeply."

Of the two sincere Republicans included in the executive Directory, I should have met one among the *Fructidorisants,* the other among the *Fructidorisés;* the satrap Barras — of whom it might have been said, without exciting contradiction, that he was always sold, and always for sale — would have offered himself to me as the friend, as the ally, or at least as the intimate confidant, of the austere, the honest La Révellière; I should have seen that same Barras, who already, perhaps, at that epoch, corresponded directly with the Count de Provence, surrounded by a crowd of myrmidons, of whom none, be it said in passing, afterwards refused the imperial livery, — upset, by incessant accusations of royalty, the only man of our assemblies who, always constant in his convictions, battled foot by foot against the insatiable ambition of Bonaparte.

Seeking in the sequel by facts, and only by facts, whether the majority of the counsellors was really factious; whether the counter-revolution could not be avoided but by a *coup-d'état;* in a word, whether the 18th of Fructidor was inevitable, I should have found, and this notwithstanding the mutual concessions which the authors of the proscription no doubt made, as in the time of Octavius, of Lepidus, of Anthony,—I should have found an elimination, or, if you will, a filtering, of forty-one members only, in the Council of the Five-Hundred, and of eleven in the Council of the Elders.

The thread that could safely guide the historian in this labyrinth of contradictory facts, I repeat it, I have not found. The memoirs snatched from the family of Barras by order of Louis XVIII.; the memoirs that were left by La Révellière, and of which it is so desirable that the public should be no longer deprived; the confessions which, on the other hand, we have a right to expect on the part of some of the victims of the Directorial *coup-d'état,* may, perhaps, dissipate all the clouds. Would to God,

for the honour of the country, that in the end, the violent and illegal mutilation of the national representation may not appear to be the exclusive result of the animosities and personal antipathies excited, or, at least, in great measure fostered, by the intrigues of several notorious women. Still the investigations of future historians, however extended and complicated they may be, can never militate against the perfect uprightness of our co-academician. Already there remain no vestiges of the accusations detailed in the official report presented in the year VI. to the Council of the Five-Hundred: in a few pages, Carnot reduced them to nought. All that malevolence or mere preconception dares to borrow now from the pamphlet elaborated with so much artifice by Bailleul, is reduced to an empty reproach coarsely expressed, and which I should have disdained to mention, had not Carnot himself indicated on what conditions he accepted it.

Political hacks call by the name of simpletons, all men who would disdain such advantages as are bought at the expense of good faith, honesty, and morality. But we must not be deceived: *simpleton* is the polite epithet; *blockhead* is preferred when we do not feel ourselves bound to keep within limits or to adhere to the language of good society. This epithet, disdainfully cast by Bailleul in the official report, had cruelly mortified Carnot; it is ironically repeated in almost every page of our colleague's answer. He says in one part: "Yes! the blockhead Aristides is chased from his country; the blockhead Socrates drank hemlock; the blockhead Cato is reduced to commit suicide; the blockhead Cicero is assassinated by order of the triumvirs. Yes! the blockhead Phocion is also led to the scaffold, but glorying in having to undergo the fate reserved in all ages for those who serve their country well."

Carnot escaped from the Luxembourg at the moment that the myrmidons were entering his room, to arrest him. A family of Burgundian artisans received and concealed him. Those whose life is an uninterrupted series of privations, know well how to compassionate misfortune. Our colleague afterwards sought refuge in the house of M. Oudot, a great partisan of the *coup-d'état* on the 18th Fructidor; and where, from that date, no one would have thought of seeking the proscribed Director. Carnot had not yet left Paris, when his name was erased from the list of the members of that national Institute, to the creation of which he had so much contributed.

Some laws proclaimed on the 19th and 20th of Fructidor, year V., declared all the places vacant that had been held by the citizens struck by the *coup-d'état* of the 18th. The Minister of the Interior, Letourneux, therefore wrote to the Institute enjoining it to proceed to the naming of a successor to Carnot. The three

classes then proceeded to the nomination of the members of each class. *One hundred and four* voters took part in the election; but the urn did not receive one white ball!

I know, Gentlemen, how much, in revolutionary times, the most upright, the most firm minds, are influenced by public opinion; I know that after the lapse of time that separates us from the 18th of Fructidor, no one can conceive that he has a right to blame the Institute at all for having yielded to the ministerial orders; still, I will here express freely my regret, that imperious circumstances did not permit our honourable predecessors, since the Fructidorian æra, to draw a marked line of distinction between the politician and the philosopher. Under the Regency, in the affair of the Abbé Saint-Pierre, Fontenelle had already, by a courageous stroke, protested against the powers attempting to confound that which the interests of science, of literature, and of art bid us keep for ever apart. If, in the year V. of the Republic, fifty-three voters had had the manliness to imitate Fontenelle, the Institute would not have suffered such cruel mutilations at the Restoration; deprived of the support afforded by unfortunate precedents, certainly not many ministers would have entertained the unpardonable thought of creating an Academy of Sciences at Paris without Monge, an Academy of Fine Arts without David!

You are surprised, no doubt, that I have not yet informed you of the name of the person who succeeded Carnot in the first class of the Institute; well, Gentlemen, it is because I have refrained, as much as possible, from performing a painful duty. When it proceeded to elect a successor to one of its founders, to one of its most illustrious members, the Institute obeyed, at least, an established law proceeding from the powers of the State; but is there, I ask you, any consideration in the world that should induce a man to accept the academic spoils of a learned victim of party rage, and especially so, when that man is General Bonaparte? Like all of you, Gentlemen, I have often indulged in a just feeling of pride, on seeing the admirable proclamations of the army of the East, signed: MEMBER OF THE INSTITUTE, *General-in-Chief;* but a heart-grief followed the first sensation, when it occurred to my mind, that the *Member of the Institute* had arrayed himself with a title which had been torn from his first patron and friend.

I have never thought, Gentlemen, that it was useful to create beings of ideal perfection, at the expense of truth; and this is the reason why, notwithstanding some friendly advice, I have persisted in divulging what you have just heard, relative to the nomination of General Bonaparte to the Institute. "But," said a Napoleonist to me, "coming from you, the story has no weight;

for does not all the world know that you astronomers seek to find spots in the sun!" Thus, Gentlemen, my position has given me the privilege of telling truth without offending any one, which, by the by, is extremely rare!

I regret not being able to discover the name of the generous citizen who snatched Carnot from his retreat, and carried him safely in his postchaise to Geneva.

On arriving in that city, Carnot engaged lodgings at a laundry-man's under the name of Jacob. Prudence required his being entirely unknown; but the wish of getting certain news from his beloved country carried the day: he went out, he was recognised in the street by some spies of the Directory, who followed him, discovered his retreat, and immediately set a watch on it. Some French agents who had influence in the Genevese Republic, exclaimed loudly that he ought to be given up to the laws of his country, and even made an official representation to the Genevese Government. The magistrate into whose hands this diplomatic affair fell, was fortunately a man of feeling, and conscientious withal, and who felt what a great blemish would be inflicted on his country thereby. This magistrate was named M. Didier. On such an occasion, Gentlemen, it would be a crime not to cite a name known also in literature, thus connected with a humane action. M. Didier wrote to Carnot; he warned him of his danger, entreated him to quit the house immediately, and directed him to a spot on the lake where a boatman would await him, to take him over to Nyon. It was already very late; the constables of the Directory were watching their prey. Our colleague goes direct to his host, and without any preamble, asks pardon for having introduced himself into his house under a false name. "I am proscribed, I am Carnot, they are going to arrest me; my fate is in your hands: will you save me?" said he. The honest laundry-man replied, "Without any doubt." Immediately he muffled up Carnot with a blouse, with a cotton cap, with a *dossier;* he lays on his head a large loose bundle of dirty linen, which hung down to the shoulders of the pretended Jacob, and hid his figure. By favour of such a disguise, the man who a short time before by writing a few lines could scatter or arrest in their march armies commanded by a Marceau, a Hoche, a Moreau, a Bonaparte; to shed hope or fear at Naples, at Rome, at Vienna; now—melancholy viscissitude of things here below,—having borrowed the trappings of a laundry-man's labourer, reaches in safety the little boat, in which he is to escape from being sent a prisoner back to France. In the boat, a new and strange emotion awaited Carnot. In the boatman appointed by M. Didier he recognises that same Pichegru,

whose culpable intrigues had perhaps rendered the 18th Fructidor inevitable. During all the time occupied in crossing the lake, not a single word was exchanged between the two proscribed men. Indeed, the time, the place, the circumstances were not suitable for political debates, for recriminations! Carnot, moreover, had soon to congratulate himself on his reserve: on reading the French journals at Nyon, he learnt that he had been deceived by a fortuitous resemblance; that his travelling companion, far from being a general, had never manœuvred anything more than his frail boat, and that Pichegru being arrested by Augereau, was expecting to be taken back to one of the prisons in Paris. Carnot was still at Nyon when Bonaparte, returning from Italy, passed through that little town on his way to Rastadt. Like all the other inhabitants, he illuminated his windows to do homage to the general.

If the plan that I have proposed to myself were to allow me at present to speak of Carnot's rare and sincere modesty, I hope his little illumination at Nyon would not be opposed to me. When he placed two candles in his window, in honour of victories to which he had contributed by his orders, or at least, by his counsels, Carnot proscribed, Carnot labouring under the menace of a forced journey back to Paris, and then of exile in the deserts of Guyana, must certainly have been agitated by far different sentiments; nor can we presume that pride showed itself in any of them.

18TH BRUMAIRE. — RETURN OF CARNOT TO FRANCE. — HIS NOMINATION TO BE MINISTER OF WAR. — HIS DISMISSAL. — HIS APPOINTMENT TO THE TRIBUNATE.

During upwards of two years, Carnot had disappeared from the arena of politics; during upwards of two years he had lived at Augsbourg under a feigned name, exclusively occupied in the cultivation of the sciences and of literature, when General Bonaparte returned from Egypt, and with a breath reversed the 18th Brumaire, a government that had never been able to take root in the country. One of his first acts was his recalling the illustrious exile, and nominating him to be Minister of War. The enemy was then at our gates. Carnot did not hesitate to accept; but a few months after, when the immortal victories of Marengo and of Hohenlinden had given an incontestable superiority to our arms, when the independence of the country was again assured, Carnot resigned his appointment. He would not consent to appear an

accomplice in the changes that were preparing in the form of the government. Accordingly, on the 16th Vendémiaire, year IX., he wrote as follows: "Citizen Consuls, I again send you my resignation; I beg you will not defer accepting it."

It is not from a trifling cause that people part thus laconically. The letter I have just given was a corollary of the earnest disputes that were daily occurring between the Republic and the Empire, in the persons of *the first Consul and the Minister of War.*

Recalled to public affairs as a Tribune in 1802, Carnot opposed the creation of the Legion of Honour. He thinks—I was going to say, he foresees—that a distinction bestowed without inquiry by the uncontrolled will of one man, will end, notwithstanding its imposing title, and according to the natural course of things in this world, by no longer being any more than the means of attaching followers, and reducing to silence a swarm of little vanities. Carnot also with all his might opposed the creation of a Consulate for life; but it was especially at the moment when it was proposed to raise Bonaparte to the Imperial Throne, that he redoubled his energy and ardour. History has already recorded his noble words; she will also say, that surrounded by old Jacobins, surrounded even by those same men who, on the 18th Fructidor, had persecuted him as a royalist, Carnot remained standing nearly *alone* in the midst of the general apostacy, as if at least to prove to the world that political conscience is not quite an empty word.

The Tribunate was soon suppressed. Carnot retired again into private life; I will not say with joy, Gentlemen, for in our colleague's bosom the virtues of a citizen always occupied the principal place; for he had hoped, that, like another Washington, General Bonaparte would avail himself of this unique opportunity to found in France order and liberty on a stable basis; for no man initiated in public affairs, and endowed with some foresight, could without uneasiness see the reins of government placed beyond control, and without guarantee, in the hands of an ambitious soldier. I shall be able at least to show you that Carnot's leisure was nobly and gloriously employed.

PUBLICATION OF THE GEOMETRY OF POSITION.

There is a story told of a young student who, almost discouraged with some difficulties inherent in the first elements of mathematics, went to consult D'Alembert, when this great geometer answered him, "*Go on, sir, go on, and faith will come to you.*"*

* D'Alembert's advice requires much explanation; as it stands it would be

The advice was good, and geometers have followed it generally: they "*go on*," also; they perfect methods, and multiply the applications of them, without pre-occupying themselves about the two or three points where the metaphysics of the science offer obscurities. Shall it be said on that account that the filling up of these gaps should be altogether neglected? Such was not Carnot's view. We have already seen him devoting the short moments of repose which his Directorial duties left him to the metaphysics of the Infinitesimal Calculus; the suppression of the Tribunate will permit him to submit to similar investigations an equally arduous question — that of negative quantities.

It often happens that, after having reduced a problem to the form of an equation, analysis offers you some negative numbers amongst the solutions sought for; for example *minus* 10, *minus* 50, *minus* 100; these solutions the ancient analysts did not know how to interpret. Vièta himself neglected them as absolutely useless and insignificant. By degrees they got into the habit of regarding negative numbers as quantities less than zero. Newton and Euler gave no other definition of them (*Universal Arithmetic*, and *Introduction to Infinitesimal Analysis*). This notion has in modern times introduced itself into the vulgar tongue: the merchant on the most petty scale understands exactly the position of a correspondent who announces to him negative profits; poetry has also given its sanction to the same thought, as we see in these two verses, by which Chénier stigmatised his political enemies, the editors of the *Mercure de France:* —

> "Which these lettered dwarfs have done, who without literature,
> *Beneath nonentity*, sustain the *Mercure*." *

Well, Gentlemen, it is a notion thus supported by the authority of the greatest geometers of modern times, consecrated by the assent of one who has, they say, more talent than Voltaire, or Rousseau, or Bonaparte, and by the assent of the generality of the public, that Carnot has combated with the keen weapons of logic.

Certainly nothing is more simple than the notion of a negative quantity, when it is attached to a positive quantity greater than

a good motto for Jesuits. It reads altogether contrary to the spirit of mathematics, where one step is made sure of before looking out for another,— where the self-secured truth is in place of any *faith*. It is meant, perhaps, to encourage the student to disregard contingent apparent puzzles; it should then be rendered, "Hold on in the path whose truth is evident to you, and after a time you will get a clearer view of those collateral circumstances which now confuse you, while looking every way at once."—*Translator.*

* "Qu'ont fait ces nains lettrés qui, sans littérature,
Au-dessous du néant, soutiennent le *Mercure*."

itself; but a detached negative quantity, a detached quantity looked upon as isolated, must it be really considered less than zero, and, *à fortiori*, inferior to a positive quantity? Carnot agreeing on this point with D'Alembert, who, most amongst the great mathematicians of the last century, occupied himself with the philosophy of science, maintains that negative isolated quantities figure in operations admitted by everybody, and in which, nevertheless, it would be impossible to suppose them beneath zero. Notwithstanding the dryness of such details, I will quote one of these operations. No one denies that

$$+ 10 \text{ is to } - 10 \text{ as } - 10 \text{ is to } + 10.$$

In order that four numbers should form a proportion, it is necessary, and, in fact, it suffices that, if the four numbers are fittingly ranged in order, the product of the extremes should be equal to that of the means. We must not be startled at this, Gentlemen; the principle I call in here, is no other than that of the famous *rule of three* of the teachers of writing and arithmetic; it is the principle of the calculation which is executed some hundreds of thousands of times daily in the shops of the metropolis. Now, in the proportion which I have just cited, the product of the extremes is + 100, as it is also of the means; therefore

$$+ 10 : - 10 :: - 10 : + 10.$$

Nevertheless, if + 10, the first term of the proportion, surpasses the second term − 10, it is impossible to suppose at the same time that − 10, the first term of the second ratio, surpasses + 10, the second term of the same ratio; − 10 cannot be, at the same time, both inferior and superior to + 10.*

* −10 is neither inferior nor superior to +10; it is equal to it; though not algebraically =; but in taking, as our author does, the sense of mathematical formulæ, − 10 is just as good and as strong in its way as + 10 in its other way. Indeed − and + are merely symbols of action one way or the other; notwithstanding the ordinary translation of minus, − being "less," whereas it simply means negative, the opposite of positive. And though it is most habitual to our ideas to consider everything in a positive light, the negative value is just as real; a correct appreciation of it only requiring the knowledge of *where the zero of the peculiar subjects treated of is placed*, which should always be one of the data in a mathematical question; thus 10 feet below the level of the sea are just as efficient as 10 feet above, and 10 degrees below any level in the thermometer are a perfect match for 10 degrees above. In fact, − 10 may be less than + 10 in our usual manner of viewing positive things; yet mathematically and truly it is not less, nor greater, but just as great. Perhaps calling a − quantity less than nothing, has occasioned a confusion of terms; for it is merely a quantity on the other side of zero, which is only a symbol of equilibrium, or of no power one way or the other. The place and value of zero depend on the class of subjects treated of, and are previously known from experience.—*Translator.*

Such is, in substance, one of the principal arguments on which our member grounds his view, that the notion of absolute or comparative magnitude should not be applied to negative quantities any more than to imaginary ones; that we cannot examine whether they are greater or less than zero; that they must be considered "*as creations of our reason, as mere algebraical forms.*"

When the genius of Descartes had shown that the positions of all possible curves, their forms, and the whole of their properties, might be exactly included in analytical equations, the question of negative quantities presented itself under an entirely new light. The illustrious philosopher himself established in principle, that in geometry these quantities only differ from the positive in the direction of the lines on which they ought to be reckoned. This profound and simple view is unfortunately subject to some exceptions. Let us suppose, for example, that it is proposed to draw from a point without a circle, a straight line so situated that the portion comprised within the circle shall have a given length. If the distance between the point from which the line is to be drawn, and the point in the circumference which it will first meet, be taken as unknown, the calculation gives two values: the one, *positive*, corresponds with the first point of the intersection of the straight line sought with the circle; the other, *negative*, determines the place of the second intersection. Now who does not see* that these two lengths, the one positive, the other negative, must be measured from the same side of the point from which the straight line was drawn?

Carnot proposed to himself to cause these exceptions to disappear. He does not admit isolated negative solutions in geometry any more than in algebra. To him these solutions, taking away their signs, are the differences of two other absolute quantities; the one of those quantities which was the greatest in the case reasoned on, only becomes the smallest when the negative root appears. In geometry then, as in algebra, the negative root taken with the sign +, is the solution of a different question from that which was put, or, at any rate, from that which it was exclusively desired to put, in the equation. How is it now that problems foreign to the particular one which the geometer wished to resolve, mix themselves up with it: that analysis answers with deplorable

* "Who does not see?" We cannot say that we do, nor can anybody else, perhaps, who has not the calculation before him. There are many ways of measuring distances about a circle; and two different lines in it amounting to the same effect can be so often drawn, that those wishing to be convinced would prefer hearing more about it; at any rate it is easier to suppose there is something misunderstood in the working of the problem, or in the meaning of its solution, than that the whole system of notation, on which all former results depend, should be wrong.—*Translator.*

fertility to questions which have not been put to it; that if its aid is sought, for example, to determine the ellipse whose area is a maximum amongst all those which can be drawn through four stated points, it gives three solutions, whilst evidently there is only one good, admissible, and capable of application; that without the knowledge and against the will of the calculator, it thus groups, in this particular case, a problem relating to the limited area of the ellipse with one concerning the hyperbola, a curve with indefinite branches, and therefore with indefinite area? Here is what required clearing up, here is that of which the theory of the *co-relation of figures* and the *Geometry of Position*, which Carnot has connected with his very ingenious views on negative quantities, give generally easy solutions.

Since these labours of our member, every one thus applies without scruple, the formula established on one particular state of any curve, to all the different forms which that curve may take. Those who will read the works of the ancient mathematicians, the collection of Pappus, for example; those who will observe, even in the last century, two celebrated geometers, Simson and Stewart, giving as many demonstrations of a proposition as the figure to which it related could take different positions or forms by the disarrangement of its parts; they will, I say, estimate Carnot's service to geometry as very high. I wish I could say, with the same truth, that the views of our member had more or less filtered into that multitude of elementary treatises which appears every year, and that they had contributed towards perfecting instruction; but on this point I can only express my regret. In the present day the philosophical part of science is very much neglected; the means of shining in an examination, or an assembly, hold the first place; with some rare exceptions, professors think much more of familiarising their pupils with the mechanism of the calculus, than of causing them to penetrate to its principles. In fact, I almost think we might say of certain persons, that they employ analysis in the same manner as most manufacturers do the steam engine, without reflecting on its mode of action. And let it not be supposed that this faulty style of instruction is a necessary sacrifice to the reigning passion of our age, the rage for going fast in everything. Have not illustrious members of this Academy shown, in justly celebrated works on geometry and statics, that extreme exactness does not exclude conciseness?

Carnot's *Geometry of Position* would not have the high merit which I have attributed to it, with regard to the metaphysics of science, if it were not also the origin and base of the progress which geometry, cultivated after the manner of the ancients, has made in the last thirty years in France and Germany. The

numerous properties of space which our member has discovered, show to all eyes the power and fecundity of the new methods with which he has endowed science. Permit me to justify by some quotations the favourable opinion which I have formed of the methods of investigation discovered by Carnot.

"If at a given point there be imagined three planes perpendicular to one another which intersect a sphere, the sum of the areas of the three circles forming the intersections will always be the same, whatever direction be given to these planes: provided that they all three cut the sphere."

"In every trapezium, the sum of the squares of the diagonals is equal to the sum of the squares of the sides which are not parallel, plus twice the product of the parallel sides."

"In every plane or uneven quadrilateral figure, the sum of the squares of the two diagonals is double the sum of the squares of the two straight lines which join the centres of the opposite sides."

I shall have attained my end if these quotations, which I could multiply to any amount, inspire professors of mathematics with the desire of seeing for themselves, in Carnot's *Geometry of Position,* how easily all these curious theorems flow from the methods of our illustrious member.

CARNOT INVENTOR OF A NEW SYSTEM OF FORTIFICATION.

There would be a gap in this biography which would justly attract your criticism, if, notwithstanding the many different points of view from which I have already considered the imposing figure of Carnot, I should neglect to speak to you of the military engineer, of the inventor of a new system of fortification.

You doubtless recollect the violent arguments which Carnot had to sustain, from the time of his entering on the military career, with the chiefs of the army to which he belonged. An upright and inflexible character already made him repel the heavy yoke of *esprit de corps.* Mature age did not contradict so honourable a début. Carnot also found, in his exalted mind, the secret of extricating himself from the sometimes rather burlesque preoccupations of men exclusively given up to one special pursuit. Even officers of engineers have not always avoided these inconsistencies. They also sometimes extend to exaggeration the consequences of an excellent principle. Some have been seen —I am certain at least of having heard so—some have been seen, who do not cross one valley, who do not surmount one hill, who do not rise over one ridge of ground, without forming the project of establishing there a large fortification, or a crenated castle, or a

simple redoubt. The idea that with the existing facilities of communication each point of the territory may become a field of battle, unceasingly besets them; it is on this account that they oppose the opening out of roads, the construction of bridges, the cutting down of woods, the draining of marshes. Fortified towns never appear complete to them; each year they add new and expensive erections to those that centuries had already accumulated. The enemy would doubtlessly have a great deal to do to overcome all the narrow and tortuous defiles, all the crenated gates, all the drawbridges, all the palisades, all the sluices for managing the water, all the ramparts, all the demilunes which unite modern fortresses; but in awaiting an enemy who may never appear, the inhabitants of some fifty large cities are deprived, from generation to generation, of certain enjoyments, of certain conveniences which render life sweeter, and which are freely enjoyed in the most obscure village.

As to the rest, harsh words shall never proceed from me, blaming the prejudices, if they are prejudices, inspired by the most noble of sentiments, the love of national independence. In everything, however, moderation is requisite. Does not economy when pushed to the extreme become hideous avarice? Does not pride degenerate into vanity; politeness into affectation; freedom into rudeness? It is by weighing in exact scales the good and the evil resulting from all human inventions, that we keep the path of true wisdom. It is thus that, despite the sovereignty of example and habit, despite the influence, generally so powerful, of uniform, the officer of engineers, Carnot, always studied important problems of fortification.

In 1788, some French officers, enthusiastic to delirium respecting Frederic the Great's campaigns, loudly proclaim the entire inutility of fortresses. Government seems to accede to this strange opinion; it does not yet order the demolition of those ancient and glorious walls; but it allows them to fall of themselves. Carnot withstands the general bias, and sends a memoir to M. de Brienne, Minister of War, in which the question is examined in all its phases with a boldness of thought, an ardour of patriotism so much the more worthy of remark, because such examples had then become very rare. It shows that in a defensive war, the only sort that he advises, the only one that he thinks legitimate, our northern fortresses might be regarded as equal to the aid of a hundred thousand men of the regular army; that a kingdom surrounded by rival nations is always in a precarious state when it has troops only without fortresses. Then, entering on the financial question, Carnot affirms (this result I am sure will astonish my audience, as it astonished me also), Carnot repeatedly affirms, that far from

being a gulf into which the treasures of the state were continually being lavished, the numerous fortresses of the kingdom from the beginning of monarchy, from the foundation of the oldest, have not cost as much as the cavalry alone of the French army during twenty-six years; and pray remark that at the time when Carnot wrote this memoir, exactly twenty-six years had elapsed without our cavalry having drawn their swords.

Well, Gentlemen, having become a member of the Legislative Assembly, this ardent advocate for fortresses proposed, whatever may have been said about it, not the complete destruction of the special and independent fortifications, backed by cities and called citadels, but the demolition only of those ramparts that before isolated them. Assuredly the certainty of there being a place of safe retreat must, in a time of siege, excite the soldier to prolong the defence and run the hazardous chance of assaults; but by the side of this advantage, citadels appeared to the mind like real Bastilles, the garrisons of which could thunder on the towns, claim ransom from them, or make them submit to any of their caprices. This reflection prevailed in the mind of Carnot, who was an eminently good citizen. The officer of engineers proscribed citadels, and, despite loud clamours, his conscientious opinion prevailed.

This is not quite the case with the new systems of fortifications, and of defence, proposed by our colleague. They have only thus far made proselytes among foreigners. Is it wrong, or is it reasonable, that our cleverest officers should reject them? God forbid that I should venture to cut short this question.* All that I can undertake is, to show in what it consists, and even to be understood I shall have to make a fresh appeal to your indulgent attention.

The most ancient fortifications, the earliest ramparts, were simple walls, more or less thick, encircling towns, and thus forming continuous inclosures pierced with a small number of gates, for the entrance and exit of the inhabitants. In order to render escalading them difficult, these ramparts were very high on the outer side towards the country; besides this, a ditch, capable of being filled with water, generally divided them from it.

The very ramparts themselves, even in their highest portion, were of a certain breadth. It was there that the population of the towns collected in cases of attack. It was from thence that, partly hidden behind a low wall now called a *parapet*, they threw a shower of missiles on the assailants. The most timid even had the

* Probably the author is alarmed at the difficulty and responsibility of deciding; otherwise he ought, as a biographer, necessarily to give some estimate of the value of all the works of the subject of his memoir.—*Translator.*

advantage of not descrying the enemy but through narrow apertures, that are still seen in modern fortifications under the name of loop-holes or *meurtrières*.

The besieger did not begin to be redoubtable but from the moment when, having reached the foot of the ramparts, he could, by means of tools, engines, or machines, sap their foundations. To act then freely and vigorously against him was, for the besieged, an indispensable condition of a good defence. Now, let us imagine to ourselves a soldier placed on the summit of the wall; he will evidently not be able to perceive the foot of it without leaning forward, without leaving nearly his whole body exposed, without losing the advantages offered him by the parapet, without the shelter of which he could not have thrown his arrows but by exposing himself to the well-aimed shots of the enemy below, Let us add that, in such an uneasy position, a man has neither power nor address. To remedy some of these inconveniences, they crowned this sort of wall with a construction which the architects called corbels, and upon which the salient parapets rested. Then the hollows, the openings, or if we must recur to the technical term, the *machicolations* comprehended between the parapet and the rampart, became a means of throwing down stones and burning substances, &c. on those who were trying to sap the walls or escalade them.

To strike the enemy unremittingly, when he reaches the foot of the rampart of a town, is undoubtedly excellent; but to prevent his advancing so far would be still better. They approached this better method, without, however, entirely attaining it, by constructing at various distances, along the wall of the city, large round or polygonal towers, forming very salient points. If we in imagination carry ourselves behind the parapet of the platforms with which those towers were crowned, it will be easy to perceive that without leaning forward, without much exposure, by much less exposure than the assailants undergo, the garrison of each tower could observe the next tower from top to bottom, and moreover a certain portion of the intermediate wall. Of that part of the wall which is now called the *curtain*, at least one half was visible down to the base by the garrison in the tower to the right, and the other half by the garrison in the tower to the left, so that there was no longer any one portion of the wall of which the besieger could approach the base, without exposing himself to the direct attack of the besieged. It is in this that *flanking* consists.

The invention of gunpowder occasioned deep-founded modifications in the system of fortification, as to the nature of attack and defence. By the aid of this invention and by that of guns of

various kinds, which arose from it, the besieger, while still at a great distance, could breach the walls by his artillery. On the other hand, the besieged gained the means of annoying the enemy long before he had reached the walls by his covert ways. Vast banks then rested against the walls, on which the largest guns could be easily worked. Thence arose the necessity of giving to the walls thus destined an immense and expensive thickness, that they might resist the thrust of these accumulations of earth. They at the same time protected the outward base of the ramparts towards the open country, by banks ingeniously contrived so as to agree with the undulations of the ground. By thus defilading the ramparts, they deprived the besieger of the possibility of making a breach from afar; they obliged him to approach very near to the body of the place, before he could expect much effect from his cannon against the walls of the besieged surmounted with guns.

It is recounted that Soliman II. held a consultation with his generals, relative to the best way of besieging Rhodes. One among them, an experienced man, explained the difficulties of the enterprise. The only answer the Sultan made was: "Advance up to me, but remember that if thou puttest only the point of thy foot on the carpet in the midst of which I am sitting, thy head shall fall." After some hesitation the Ottoman general thought best to raise the fearful carpet, and roll it on itself, in proportion as he advanced. He thus safely got closely up to his master, who then exclaimed: "I have now nothing further to teach thee; thou knowest the art of besieging." Such is, in fact, the faithful image of the first movements of the besieger, who wants to get possession of a fortress by a regular attack. The ground represents the Sultan's carpet.* His life is endangered unless he advances under cover; but let him dig the earth; let him heap up the rubbish in front of him; let him unceasingly roll up a little of the carpet as he advances; and behind this moveable shelter the besiegers, carrying with them a powerful artillery, approach the ramparts of a fortress very soon in full force, without being seen by the besieged.

The problem of fortification may, indeed, be considered at bottom as a particular case of the geometrical theory of polygonal stars. This assemblage, apparently so inextricable, of salient and of re-entering angles, of bastions, of curtains, of demilunes and

* This is scarcely a "faithful image," and unless the story could be improved, it is hardly worth preserving. The gradual increase of labour and cover in advancing, and the total absence of cover at the commencement, are features foreign to a regular siege; there cover is obtained at first, and they go steadily on, making no more or less than the requisite cover all the way. The story, however, might be applied to an old fortress with very lofty towers, which would require more cover as you got nearer.—*Translator.*

ténailles, &c., of which modern fortresses consist, is the solution of the very old question of flanking. We may in some points vary the construction, but the aim is always the same. The abstract principles of the art have become clear and evident. The illustrious corps of officers who at the present day are at liberty to apply them to the defence of the country, have had the good sense to abandon the mystery with which it was before surrounded, and with which it has been so severely reproached. Fortification is taught like other sciences; it is founded on the most elementary geometry; a mere amateur can familiarise himself with the theory in a few lessons.

Let us now remark, that modern fortification has the defect of being extremely expensive. It was this ruinous defect that Carnot wished to remove, by substituting curved (*or vertical*) fire for the direct.* Carnot surrounds a fortress by a simple wall, not faced, but furnished with scarp and counterscarp. The wall does not require a great thickness, because it has not to resist the thrust of the earth destined to bear artillery on it. Behind this wall he places mortars, howitzers, and *pierriers* which are to carry curved fire into the country; the results of which, according to him, must be much more effective than those of direct firing, and oppose obstacles to the enemy's advance, more and more efficacious in proportion to his approach. The wall is defiladed against the direct fire of the besieger, by the earthen counterscarp, forming one of the faces of the ditch. It seems, then, that to make a breach, it is requisite, as in the present system of fortification, to crown the covered way; an operation which, according to the author, would be eminently galling to the assailant. This supposes that a breach could not be made in Carnot's wall but at a very short distance, and within *le tir de plein fouet*, or point-blank range. Foreign experiments, it is said, contradict this hypothesis; by employing curved fire, a breach might be made at a sufficiently great distance, provided the projectiles were of very large calibre. The question then is not yet solved †; the new mode proposed by

* The word "curved fire" is employed, though "vertical" is the usual term in English technical language, because *curved* includes more — as the vertical, the ricochet, and everything between those two. It might possibly be rendered "elevated fire;" and it should be remembered that Carnot intended to use a sort of ricochet fire as well as the vertical.—*Translator.*

† We should say, the question *is* now solved; the experiments made by *foreigners* are to be relied on, and are kept on record; at least that part which he speaks of, namely — the impossibility of breaching Carnot's wall from a distance. In the experiments made at Woolwich, a wall well-built, and having had time to consolidate, *was breached* with expedition and certainty; though of course with a very large expenditure of ammunition, on account of the uncertain nature of the fire; that is, throwing heavy shot over an earthen bank, down

Carnot seems to call for a more thorough examination; but meanwhile we must applaud our illustrious colleague for his endeavour to render the means of defence as efficacious as the means of attack, which were due to the genius of Vauban.

PUBLICATION OF THE TREATISE ON THE DEFENCE OF FORTRESSES.

Napoleon was greatly irritated in 1809, at the slight resistance that several fortified places made to the attacks of the enemy; and therefore he caused Carnot to be asked, towards the end of that year, to write a special code of instructions on this important branch of the military art, from which the governors of citadels might learn the responsibility of their functions, and the full extent of their duty. In this mission Carnot saw a fresh opportunity of rendering himself useful to his country, and did not hesitate to

against the wall on the opposite side. The vertical fire question does not admit of quite so easy a solution; but Carnot certainly miscalculated the effect of the very small balls he proposed to shower down, as is immediately evident theoretically, and has been tested practically. He said that, a large ball fired at a certain angle with a certain velocity, being found to penetrate on falling into hard earth, about its own diameter, his small balls fired under like angles and velocities would also penetrate to the amount of their diameters; but this is fallacious, he having forgotten the resistance of the air, which retards balls of different sizes in the proportion of the *squares* of their diameters, while their force, or power of retaining momentum, is in proportion to the *cubes* of their diameters. This is an immense difference when it is recollected that Carnot's given experiment was with a ball of some five or six inches diameter, while those he proposed would have been of about one inch; and that in vertical fire this resistance of the air acts on the ball through a lengthened route both ascending and descending. Experiment with the proposed balls at Woolwich, has shown that the wounds inflicted by these balls would be seldom disabling, unless they struck a man on the head; their force being only somewhat greater than the strongest effort of a strong man. It has also been shown, that they are given to scatter so much, that the *outworks* in the neighbourhood must be abandoned as soon as this fire is used from the *body* of the place; in fact, by making vertical fire the whole of his defence, Carnot forfeits all the time which the use of direct fire used to cause the assailant to expend in approaching to the summit of the covered way, as a very slight application of raw hides, &c., supported above the approaches, would protect the assailants; and when there, the neighbouring works could not assist in defence, as they must be abandoned from the scattering fire from the body of the place.

Still vertical fire is often good and effective, especially in the latter parts of sieges; and all writers on fortification recommend its extensive use both in defence and attack. Its use has been restricted by the difficulty of transporting ammunition, or the train being borrowed from ships, or its being incomplete; but the advantage is allowed by all writers, though only as a part of the system. Carnot's principal novelty was the *theory* of making it take the place of everything else; and that theory has been ably demolished by the practical arguments of Sir Howard Douglas.—*Translator.*

accept it, although his health then occasioned some serious inquietude.

In the eyes of the Emperor, perhaps *working fast* was more esteemed than *working well.* On this occasion, however, his hopes did not go so far as to imagine that the composition of a considerable work that might require ten or twelve large plates, and in which some well-selected historical examples should accompany and support the precepts, could be executed in less than a year. Well, Gentlemen, four months scarcely elapsed from the moment that Carnot knew Napoleon's desire, to the publication of the celebrated Treatise on the Defence of Fortresses.

CARNOT AN ACADEMICIAN.

From 1807 to 1814 Carnot had lived in retirement; he scrupulously fulfilled his duties as an academician. This title had been restored to him the 5th Germinal, year VIII., after the decease of Le Roy. Nearly all the Memoirs on Mechanics submitted to the judgment of the First Class of the Institute, were referred to him. His rare sagacity, with luminous clearness and remarkable precision, pointed out and characterised the new and salient portions. I could cite a certain author on machines, who did not fully conceive his own discovery, until after it had had the good fortune to pass through the filter of that learned critic. He had, besides, a sort of merit that is not always the auxiliary of high science: he knew when to doubt; to his eye theoretical results were not always infallible.

EVENTS OF 1813. — CARNOT APPOINTED TO THE COMMAND OF ANTWERP.

We have now reached the events of 1813. Carnot was not rich enough to subscribe to the newspapers. Every day at the same hour, we see him come to the Library of the Institute, approach the fire, and read with visible anxiety the news of the progress of our enemies. On the 24th of January, 1814, the interest he felt appeared greater than ever; he asked for some paper, and as fast as the pen could trace, wrote the following letter, which you will no doubt like to hear read: —

"SIRE, — As long as success crowned your enterprises, I abstained from offering to Your Majesty services which I did not think were agreeable to you; now, that ill-fortune puts your constancy to a severe proof, I no longer hesitate to offer you the small means that remain to me. It is little, certainly, to offer a sexagenary arm; but I have thought that the example of a soldier

whose patriotic sentiments are known, might rally to your Eagles many men who are undecided what line to adopt, and who may allow themselves to be persuaded, that in abandoning them, they were serving their country.

"There is still time for you, Sire, to conquer a glorious peace, and *to have the love of the great people restored to you.*—I am, &c."

The details that I have thought it right to give you, relative to the circumstances connected with the writing of this letter, will, I trust, undeceive those who, accustomed to concentrate all their affections on the person of Napoleon, fancied in Carnot's concluding words, a cruel attack from the old democrat, prepared at long-shot distance, against the man who had confiscated the Republic to his own advantage. In truth, Gentlemen, it required a man to be very determined to substitute personal questions for the national weal, to blame the illustrious sexagenarian's offer to defend a fortress, when otherwise he had, relative to capitulations, not long since resumed his idea, expressed in the noble words of the famous Blaise de Montluc to Marshal de Brissac: *I would rather be dead than see my name in such writings.*

Carnot started from Paris for Antwerp at the end of January, without having even seen the Emperor. It was time, Gentlemen, for the new governor could not reach the fortress on the morning of the 2nd of February, but by threading the enemy's bivouacs. The bombardment of the town, or rather the bombardment of our fleet, for there were some English among the besiegers, began the next day; it lasted throughout the day of the 3rd, and of the 4th, with part of the 6th. Fifteen hundred bombs, eight hundred common balls, many red-hot balls and fusees, were thrown on our ships. The enemy then retired; the experience of three days had sufficed to give him the estimate of the rough tilter he had to deal with.

I will borrow from the journal of the siege kept by M. Ransonnet, Carnot's aide-de-camp, some details that may be interesting, and which will show the strictness of the man and of the times.

On the 10th of February, the new governor of Antwerp wrote to the Mayor of the town:—

"I am very much surprised that the person charged with ordering the furnishing of my quarters, did not restrict himself to what was necessary.

"I also desire that any demands of this nature, made on my account, may not have the character of a forced requisition.

"All the effects enumerated in the annexed list are unnecessary."

The necessities of the Belgian campaign, having suggested to the Emperor the idea of borrowing some troops from the garrison

of Antwerp for the army, Carnot wrote a despatch to the General-in-Chief, Maison, dated the 27th of March, whence I have extracted the following passages:—

"In obeying the orders of the Emperor, I am obliged to declare to you, the Commander-in-Chief, that these orders are equivalent to ordering Antwerp to be ceded . . . . The circumference of this place is immense, and there would be at least fifteen thousand good troops required to defend it. How could His Majesty suppose that with three thousand sailors, the greater part of whom have never seen fire, I could keep the fortress of Antwerp with the eight dependent forts? . . .

"It only remains then here, for us to disgrace ourselves or to die; I beg you to believe that we are all determined to prefer the latter . . . .

"I think, Sir, that if you could take upon yourself to leave me at least the infantry and the artillery (there was at Antwerp a detachment of the Imperial Guard), you will render a very great service to His Majesty; but all will be ready to depart to-morrow, unless I receive a counter-order from you, which I shall await with the greatest impatience and the greatest anxiety."

Besides the despatch to General Maison, I find under the same date a letter to the Minister of War, the Duke de Feltre; and I remark the following passage in it:—

"When I offered to serve His Majesty, I was willing to sacrifice my life to him, but not my honour. Your Grace knows that I am not in the habit of dissimulating the truth, because I do not seek favour. The truth is, that the state to which your orders reduce me, is a hundred times worse than death, because I have no chance of saving the place confided to me, but in the cowardice of my enemies."

Bernadotte having wished to dissuade Carnot from the line of conduct that he had laid down for himself, received from him the following answer:—

"10th April, 1814.

"Prince,—It is in the name of the French Government that I command in the fortress of Antwerp. That Government alone has the right to fix the termination of my duties: as soon as it shall be incontestably established on a new basis, I shall hasten to obey its orders. This resolution cannot fail of obtaining the approbation of a Prince born a Frenchman, and who knows so well the laws prescribed by honour."

After the events of Paris, after the institution of a Provisional Government, the Minister of War, Dupont, sent one of his aides-

de-camp to Antwerp. The following is the letter that Carnot wrote to him on this occasion:—

"15th April, 1814.

"I must acknowledge, my Lord Count, that your having sent me an aide-de-camp with a white cockade is a calamity: some wished to adopt it instantly, others have sworn to defend Bonaparte; a sanguinary conflict would have immediately resulted in Antwerp itself, if, with the advice of my Council, I had not determined to defer my adhesion, and that of all the armed force . . . Is civil war then wished for? is it wished that the enemy should become master of all our strongholds? and because the city of Paris has been forced to accept the rule of the conqueror, that therefore all France shall receive it? It is evident that the Provisional Government is only transmitting the orders of the Emperor of Russia. Who will ever absolve us for having obeyed such orders? What! you do not allow us even to preserve our honour; you yourself become an accomplice of desertion, promoter of the most monstrous anarchy! The lessons of 1792 and 1793 are lost upon the new chiefs of the State. They first seek to catch our adhesion by surprise, by affirming to us that Napoleon had abdicated, and now they contradict it. After having given us a tyrant instead of anarchy, they put anarchy in place of the tyrant. When shall we see the end of these cruel oscillations? Paris is enjoying only a momentary peace; a perfidious calm which presages to us the most horrible tempests. Oh, afflicting and withering days, happy those who have not witnessed you!"

The sentiments with which Carnot was able to inspire the population of Antwerp are known to the whole world. I cannot resist the pleasure, however, of citing at least some passages from a letter that was delivered to him the day he departed for Paris; after having been thus ordered by the government under the elder branch of the Bourbons, who had remounted the throne. The authorities and inhabitants of the suburb of Borgerhout, the destruction of which had been resolved on, but which he thought he could preserve without detriment to the general defence, thus expressed themselves:—

"You are going to quit us; we feel deeply afflicted by it; we would wish to possess you still a little longer; we solicit this great favour most earnestly . . . . . The inhabitants of Saint Willebrord and of Borgerhout request permission to inquire once a year, of the person who may be appointed to govern them, after the health of General Carnot . . . . We may, perhaps, never see you again. If some day General Carnot allows his portrait to be

painted, and would permit a copy of it to be made for us . . . this precious present would be deposited in the church of Saint Willebrord."

I will not commit the fault, Gentlemen, of weakening such naïve and touching expressions by a cold commentary.

CONDUCT OF CARNOT DURING THE HUNDRED DAYS.

The conduct of Carnot during the Hundred Days, appears to me to have been well and honourably epitomised in those memorable words that Napoleon addressed to him, after the battle of Waterloo: "CARNOT, I HAVE KNOWN YOU TOO LATE!"

But, as I am writing a biography and not a panegyric, I will frankly say that Carnot, as member of the Provisional Government of that epoch, laboured under the injurious and anti-national influence of the Duke of Otranto, which led him to give his adhesion to measures that were stamped with evident feebleness, and to others over which every heart animated by patriotic sentiments would gladly throw a thick veil.

And yet, can we very warmly reproach Carnot with having allowed himself to be fascinated by the intrigues of Fouché, when we see Napoleon, notwithstanding the strongest suspicions of treason, retain that man in his Council?

Amidst the reproaches ostensibly addressed to Carnot, respecting that period of our annals, there is one on which I can give some personal explanation. I have heard the austere Conventional severely blamed for having accepted the title of *Count of the Empire:* happily my memory can faithfully repeat some words of our colleague's which clear up this point in his life, and which were related to me by an officer the very day that he heard them.

They were at table, at the Minister of the Interior's house. A letter arrives; the Minister breaks the seal, and almost instantly exclaims: "Well, Gentlemen, see me here a *Count of the Empire!* I can, however, easily guess *whence the blow comes.* It is my dismissal that is wished for, that is demanded; I will not give *him* that satisfaction; I will remain, because I think I can be useful to my country. The day will come, I hope, when I shall be allowed to explain myself fully respecting this perfidy; at present, I will content myself with disdaining this vain title, with never annexing it to my name, and especially with never accepting the diploma, however much I may be pressed to do it. From this moment, Gentlemen, you may rest assured that Carnot will not long remain Minister after our enemies have been repulsed."

I must have made you ill-appreciate our colleague, Gentlemen, if these words had appeared to require farther explanation.

CARNOT IN EXILE. — HIS DEATH.

Of all the ministers of the Hundred Days, Carnot was the only one whose name appeared on the list of *proscription* prepared on the 24th of July, 1815, by the second Restoration. Whether this special rigour was the consequence of the patriotic ardour with which our colleague disputed with foreigners the last remnants of the French territory, or of his persisting (though unfortunately without good result) to point out to the Emperor the traitor, who, under the favour of his former reputation for talent, had insinuated himself into the Ministry, still his glory will not be tarnished by it.

Already, on the evening of the 24th July, Carnot had received a passport from the Emperor Alexander. He used it, however, only in Germany. Obliged to travel under a feigned name, he would not forego the title of a Frenchman as long as he could avoid it. It was therefore again as a Frenchman that he traversed the great river in a melancholy mood, to the very banks of which he had had the supreme honour of extending our frontiers, and he went to Warsaw.

In a certain country not far from ours, a stranger is always received with this matter-of-course formula: "My house, and all that it contains are yours;" but at the same moment, I must acknowledge, it is not rare, through a signal that the servants perfectly well understand, for the supposed new *proprietor* to be ever after shown the door, of the habitation so liberally offered to him. The reception of Carnot in Poland, however, must not be included in this category. Our excellent friends, the brave Poles, did not confine themselves to mere forms of politeness towards the illustrious exile. General Krasinski made over to him a mayoralty in land of 8000 francs per annum, that he held from Napoleon; the Count de Paç wished him to accept the possession of several domains. Although Carnot was not a Free-mason, all the Masonic Lodges of the kingdom joined in a subscription that produced a considerable sum; finally, and of all these offers that he refused, the following went most directly to the heart of Carnot; a Frenchman, poor himself, established at Warsaw for many years, went to him one morning, carrying a bag with the savings of his whole life!

The severity of the Polish climate, the wish to be nearer to France, determined our colleague to accept the kind offers of the Prussian Government: he settled at Magdebourg, where he passed his latter years in study, in meditation, and in the company of one of his sons, whose education he superintended. It was, Gentlemen, a fine spectacle to see the whole of Europe,

above all to see the absolute monarchs, forced in some measure to render homage to one of the greatest, most noble, most striking men in the French Revolution; even to one of the judges of Louis XVI., even to a member of the *Committee of Public Safety.*

Carnot died at Magdebourg, the 2nd of August, 1823, aged seventy years.

PORTRAIT OF CARNOT. — ANECDOTES RELATIVE TO HIS POLITICAL AND PRIVATE LIFE.

If *iconography* is not now considered by anybody as a futile science, if some very distinguished minds have made it the object of their earnest study, it may be permitted me here to say, that Carnot was of tall stature, of manly and regular features, a wide and calm forehead, lively and penetrating blue eyes, a polite demeanour, but circumspect and cold; that at the age of sixty, there was still perceptible in him, even in a civilian's costume, something of the military air to which he had been accustomed in his youth.

I have considered him in all his phases,—as a member of the Conventional Government, of the Committee of Public Safety, of the Executive Directory, the Minister of War, a Military Engineer, the Exile, the Academician. Still many essential traits would be wanting to the portrait, however comprehensive it be already, if I did not also speak of the private man. I shall not swerve, in this latter portion of my picture, from the style that I adopted in the beginning; I shall advance always proof in hand. It is thus I think that a geometer should be praised; I mistake, it is thus that everybody should be praised: seeing how rare honour, disinterestedness, and true patriotism are among the living; and how common, on the contrary, among the dead, according to their funeral eulogies and their epitaphs; the public has come to the wise conclusion of no longer believing either the one or the other.

I have read somewhere that Carnot was an ambitious man. I will not stop to combat this opinion in form, but I will relate, and you yourselves shall judge.

The member of the Committee of Public Safety, who, in 1793, organised the fourteen armies of the Republic, who arranged all their movements, who named and appointed generals, who at need, as at Wattignies, degraded them during the battle under the enemy's cannon, — was only a Captain of Engineers.

And later, when the Council of the Five Hundred, and the Council of the Elders of the Republic of the year III., unanimously called Carnot to the Executive Directory; when having again become the supreme arbiter of our military operations, he

he sent Hoche to la Vendée, Jourdan to the Meuse, and Moreau to the Rhine, instead of Pichegru; when, by the most fortunate inspiration, he confided the command of the army of Italy to Bonaparte, our colleague gained a step, but only one step; he had become chef de bataillon *by seniority!*

Carnot still held only this humble rank, when the coup d'état of the 18th Fructidor banished him from France.

The extremely hierarchical ideas of the First Consul could never have reconciled themselves to a mere chef de bataillon being Minister of War. Wherefore in the year IX., he did not elevate Carnot to that eminent post until after he had named him Inspector General of Reviews. Still it was only turning the obstacle aside, instead of removing it. The semi-military, semi-civil, grade of Inspector General of Reviews, did not prevent the Minister of War, under the government of the Consuls, from being a simple *chef de bataillon* in the Corps of Engineers.

Carnot quitted the Ministry the 16th Vendémiaire, year IX. Twelve days after, his successor asked for the illustrious citizen's name to be inserted in the list about to be formed of the Generals of Division of the French army. The Report recalled in appropriate terms, and even with a degree of vivacity, all that our colleague had done for the national glory and independence. The Minister went, even in the name of *justice*, of *esteem* and of *friendship*, to invoke the *magnanimity* of the Consuls: the magnanimity was at fault; they did not answer the report, and the dismissed Minister remained in his old rank.

When it was requisite, in 1814, to send orders to the new Governor of Antwerp, the Clerks of the War-office, in order to write the address, sought for the official titles of Carnot in the Army-list, and were astounded at seeing that the Emperor had, without considering it, placed a chef de bataillon at the head of a crowd of old generals. The service would evidently have suffered from such a state of things; the necessity of remedying it was at felt, and, in imitation of a certain ecclesiastical personage, who in the same day received the minor orders, the major orders, priesthood, and episcopacy, our colleague, in a few minutes, passed through the various grades of lieutenant-colonel, colonel, brigadier-general, and general of division.

Yes, Gentlemen, Carnot had ambition; but, as he said himself, *it was the ambition of the three hundred Spartans going to defend Thermopylæ!*

The man who, in an all-powerful position, had never thought of making himself the equal of those whose vast operations he was directing, also disdained the gifts of fortune. When he returned to private life, his small patrimony was scarcely intact. How is

it, with the most simple tastes, with a strong antipathy for pageantry and show, that Carnot even unintentionally does not reach, if not riches, at least the easy circumstances of those men who, like him, have long held brilliant employments? Some facts will serve as answers.

After the 18th Brumaire, at the moment when Carnot became Minister of War, the pay of the troops, and, what must occasion still more surprise, the pay of the clerks, was fifteen months in arrear. A few weeks elapsed and all was paid up; all, except the salary of the Minister himself!

Pins, was the name given to a sort of gratuity destined in appearance for the wife of any one with whom a farmer, a merchant, or a commissary had concluded a contract, whether public or private. Although pins did not appear in the written conditions, the contracting parties did not therefore regard them as less obligatory; habit, that second nature, had at last come to acknowledge them as legal; the most sensitive consciences satisfied themselves by not fixing their amount.

A horse-dealer, whose offer Carnot had approved, was going, according to custom, to bring him a considerable sum, under the name of pins; it was, I believe, 50,000 francs. The Minister, at first, does not understand. At the Committee of Public Safety, where he had served his apprenticeship, the purveyors took good care not to speak of pins. All is explained at last, and Carnot, far from being angry, receives with a laugh the notes that are presented to him; he receives them with one hand, and gives them back with the other, as a first instalment of the price of the horses that the dealer had agreed to furnish for our cavalry, and demands an immediate receipt.

In the most violent paroxysms of their fury, the factions had the prudence not to attack Carnot as a private man; never did their unhallowed breath try to tarnish the virtues of the son, of the husband, or of the father; as to disinterestedness especially, both friends and enemies were always agreed. I might therefore on this point remain content with the two instances I have given. There is another, however, which it is desirable to rescue from oblivion; the memory of Carnot does not require it, but I have a slight hope that, by being reminded of it, some ministers may feel arrested in their prodigalities, and certain parties from indulging their avarice!

After the 18th Brumaire, the projected operations for the army of reserve imperiously required that Moreau should without delay send one of his divisions to the army of Italy. The direct intervention of the Minister of War did not appear too much to carry so important a negotiation to a successful conclusion. In execution

of an order of the Consuls, of the 15th Floreal, year VIII., Carnot, accompanied by six officers of the staff, *two* couriers, and *one* servant, went to Germany. On the route he inspected the troops *échelonnées* between Dijon and Geneva; he then traversed the cantonments of the Rhine, visited the fortresses, arranged with the Commander-in-Chief the plan for the next campaign, and returned to Paris. The Treasury had given him 24,000 francs. On his return, he restored 10,680 francs. He was so fearful that the expenditure of 13,320 francs, (or £550), for *ten* persons making a long journey should appear too much, that he sent in a detailed report, excusing himself as if he had been prodigal. The following was his letter to the Consuls: "You will have the goodness to remark that you have desired me to give some éclat to my mission; that in the principal places I was obliged, according to your orders, to assume a certain appearance; in short, that it was requisite, from the character of generosity with which you are animated, that I should allow some gratification to my companions in travel and in fatigue!" Be pleased to remember, Gentlemen, that the journey, the éclat, the gratifications amounted altogether to 13,320 francs; do not forget that it was one of the ministers, inspecting armies who was going to decide on the fate of his country, who spoke thus, and you will agree with me, I think, that if the world is improving, it is not in economy.

The Treasury did not know under what form to record the 10,680 francs returned by Carnot; but it was not the first essay on the part of our colleague: by searching back to the epochs when he inspected the Republican armies, as representative of the people, the Clerks of Finance found in their registers the forms they sought, and these occurred as often as Carnot had executed similar missions.

The name of Carnot would still present itself to my mind if, after so many instances furnished by history in all countries, it were yet required to prove that an ardent mind can be allied to cold and reserved manners. Undoubtedly, no one ever had a right to say of him, as D'Alembert said of one of the old secretaries of our Academy: *He is a volcano covered with snow;* but I may be allowed to show at least, that our colleague's conceptions often had a certain something in them that went direct to the heart, touching, moving, electrifying; something, in short, stamped with an indefinable seal, never borne by the works of heartless men, of men whose faculties have no concentration of mind. Two more citations, and my thesis will be proved.

Latour of Auvergne, born of the Turenne family, did not even express regret at losing his advantageous position through the breaking out of the Revolution; but when the enemy

menaced our frontiers, it was to the frontiers that he was seen to march. Modesty made him decline all promotion; the old captain obstinately remains a captain. In order not to deprive the country of the eminent services that Monsieur Latour d'Auvergne could render it, Carnot authorises the representatives of the people to group together all the companies of Grenadiers of the army of the Western Pyrenees, and form a separate corps of them; never to place a senior officer over them, and to remove with equal care all the captains that were senior to Latour d'Auvergne; by this arrangement the diffident officer finds himself daily in charge of an important command. The name of *infernal column* given by the Spaniards to this body of troops soon sanctions in a splendid way all that there was of anomalous, of unusual, and strange in the contrivance suggested by Carnot, and carried into effect by the representatives.

Latour d'Auvergne, whom you now know, Gentlemen, as a military man, for the third time quitted his retreat and his beloved learned studies, and asked to serve under Moreau, when Carnot became Minister of War after the 18th Brumaire. Already at that epoch the First Consul would not certainly have approved an arrangement similar to the one that the Conventional representatives adopted in the Pyrenees. Carnot, however, suffered in seeing that the chief of the *infernal column*, he who counted so many dashing services, that the estimable author of the *Gaulish Origins* — must we add, that a correspondent of the Institute, should arrive on the banks of the Rhine as an obscure officer. The title of *First Grenadier of France* strikes his imagination; Latour d'Auvergne is invested with it by an official act; and from that moment, without quitting his Grenadier epaulettes, he became, in the eyes of the soldiers, the equal if not the superior of all the dignitaries in the army.

The First Grenadier of France was killed by a lance the 27th of June, 1800, at the battle of Neubourg. The army, the whole of France, wept bitterly over this loss. As for Carnot, his deep grief inspired him with an idea that the ancients, otherwise so idolatrous of military glory, might envy us. By an order emanating from Carnot, when the 46th demi-brigade was mustered, the name of Latour d'Auvergne was always called out as the first on the roll. The grenadier placed at the head of the first rank then advanced two steps, and answered in a tone to be heard all along the line — *Died on the field of honour.*

The brief, expressive, solemn homage that a regiment thus daily paid to him who had rendered himself illustrious in its ranks by courage, knowledge, and patriotism, must, I think, continue that excitement which produces heroes. I assert, at all events,

that the noble words of Carnot, repeated in the chamber, in the guard-room, under the tent, in the bivouac, had thoroughly preserved the remembrance of Latour d'Auvergne in the memory of our soldiers. "Where are those long files of grenadiers going?" exclaimed the aide-de-camp of Marshal Oudinot, when, in the beginning of Vendémiaire, year XIV. (October 1805), the avant garde of the great army passed through Neubourg. "Why are they swerving from the route laid down for them?" Their silent and grave march awakened curiosity; they are followed, they are observed. The grenadiers were going, Gentlemen, near Oberhausen, thoughtfully to pass their sabres over the rough block of stone that covered the body of *the first Grenadier of France.*

I return thanks, Gentlemen, to M. de Savary, the venerable old man, who, a witness of the touching scene near Oberhausen, has allowed me to draw it from oblivion, and thus to unite in one mutual sentiment, the admirable army of Austerlitz with the admirable armies of the Republic. I am happy also, that names which are dear to you, that the names of two of our old colleagues, that the names of Latour d'Auvergne and of Carnot, happen to occupy so noble a place in this patriotic reminiscence!

Great employments, like great heights, usually occasion a vertigo in the heads of those who reach them suddenly. This man thinks that by pageantry and prodigality he ought to make people forget the years he has passed in mediocrity and constraint. That man becomes disdainful and insolent, harsh and churlish, and thus revenges himself on the unfortunate people who have now to solicit him, for the disdain, the arrogance, the brutality that he had to undergo when he had to solicit them. A crowd of names of individuals suggest themselves to fill up this sketch, in case any one should dispute its fidelity. Do not suppose, however, that by passing over some mushrooms so lightly, I intend to constitute myself the advocate of privilege; I wish to prove, on the contrary, by the example of Carnot, that minds of a certain temperament can resist contagion.

Six months after the coup d'état, on the 18th Fructidor, Carnot is officially accused to the Council of the Five Hundred of having had frequent and intimate communications with Pichegru, at a time when that general, a member of the Legislative Body, soiled his brilliant military reputation by his intrigues. Carnot denies such communications. He proves besides, that he could not have had secret interviews at his house. He added: "I feel that people will say if it was not at your house it was elsewhere. Well, I declare, that during all the duration of my directorial functions, I have *not gone out twelve times* without being accompanied by my wife, my sisters, or my children!"

It is possible, Gentlemen, that in France, that elsewhere, men in power may have had this simplicity of habits, not to say integrity; but I will acknowledge it, the rumour has not reached me.

I have been speaking to you of the Man; now I will treat of the Minister.

At the battle of Messenheim (1800), near Inspruck, Championnet remarks the temerity, the intrepidity of Colonel Bisson, and demands for him, with the applause of all the army, the epaulettes of a General of Brigade. Weeks elapse, and the commission does not arrive. Bisson grows impatient, goes to Paris, obtains an interview with the Minister, and in his anger, apostrophises him in a rough manner. "Young man," Carnot calmly replied to him, "it is possible that I may have committed an error; but your improper manners, really, might disincline me to repair it. Go, I will attentively examine your services." "My services! Ah! I know too well that you despise them, you, who from the shelter of your cabinet coolly send us the order to die. Protected from danger, and from the rigour of the seasons, you have already forgotten, and you will continue to forget, that our blood flows, and that we lie on the hard——" "Colonel, this is too much! For your own interests, our interview must not continue in this tone. Retire! Your address, if you please? Go! you will shortly hear from me."

These last words, pronounced in a solemn tone, unsealed Colonel Bisson's eyes. He runs to a devoted friend, General Bessières, to seek consolation. His friend, on the contrary, gives him to understand that a court-martial will be the inevitable consequence of his folly. In the meantime Bisson hides himself. A faithful servant goes every hour to the hôtel, to learn about the dreaded order for his appearance. The ministerial paquet at last arrives: Bisson, all emotion, tears open the envelope. The paquet, Gentlemen, contains the brevet of General of Brigade, and letters of service!

It is scarcely necessary to add, that the new general flies to Carnot immediately to offer him the homage of his admiration, and of his gratitude, and of his deep repentance. All this proved superfluous, for General Bisson found his orders at the door of the Minister's office. That ardent soul which, notwithstanding all its sincerity of conduct, felt the act somewhat onerous, proved how well he had appreciated the delicate severity of Carnot, and how worthy he was of it, by that very evening publishing the details, which assuredly Plutarch himself would not have disdained.

Of all the qualities that great men can adorn themselves with, diffidence seems the least obligatory; therefore the more credit

is given to them for it; and therefore also it leaves the most durable recollections. Who, for example, does not know by heart that letter which Turenne wrote to his wife, a hundred and seventy-nine years ago, on the day of the celebrated battle of the Dunes: "The enemy came to us: they have been beaten; God be praised. I have worked a little in the course of the day; I wish you good night, and will go to lie down."

Equally with this illustrious general of Louis XIV. did Carnot omit his own participation, both in his private communications and when he wrote to the Convention. I have related to you the part he acted at the battle of Wattignies; well, read the bulletin which that decisive and memorable event inspired him to write, and you will in vain seek a few words to recall the representative of the people; unless, indeed, we are determined to see them in this passage: "The Republicans charged forward with the bayonet, and remained victorious."

But all of you, who knew Carnot, will agree with me, that unless he was pressingly and directly solicited, he would never entertain you with the European events which he had so often directed. Justly jealous of the esteem of France, the old Director, during his exile, answered the diatribes of his accusers in writing. His style on these occasions was lively, poignant, and cut deep; it was evident at each line that it proceeded from an ulcerated heart. Yet the most legitimate irritation never led him beyond the circle that his enemies had traced out. His defence in some parts might resemble an attack; but at bottom, on close examination, it was still a defence. Carnot rejected far from him, the idea of raising a pedestal to himself with the immortal trophies that he had reaped during his Conventional and Directorial career. Modesty, Gentlemen, is a good alloy when it triumphs thus over anger.

In regard to science, the illustrious academician was not less reserved. One would have said, indeed, that he regulated his conduct according to that reflection of the oldest and most ingenious of your interpreters: "When a learned man speaks to instruct other men, and exactly in that line of instruction that they wish to acquire, he does them a favour; but if he speaks only to show off his own learning, they do him a favour in listening."

Modesty, moreover, is not a quality deserving of respect and esteem, except in isolated individuals. Bodies of men, and especially academies, would be guilty of a fault, and would be wanting in a principal duty, if they neglected to adorn themselves in the eyes of the public with the legitimate claims they have earned to the esteem, gratitude, and admiration of the world. The more justly celebrated they are, the stronger is the desire to belong to

such institutions, and the more the laborious efforts made to attain this aim turn to the advantage of science, and to the glory of the human mind. This thought has encouraged me, Gentlemen, to unroll to your eyes, in all its details and in its true colours, the very eventful, varied, and stormy life of Carnot. For nearly two centuries the Academy of Sciences conscientiously has preserved the memory of the geometers, the physicists, the astronomers, the naturalists, who have rendered it illustrious. The name of the great citizen who, by his genius preserved France from foreign dominion, has appeared to me to deserve being inscribed with some solemnity in this glorious Pantheon.

MALUS.

A Biography prepared for the Public Sitting of the Academy of Sciences, 1854; and read by special Desire of that learned Body, on the 8th of January, 1855.

BIRTH OF MALUS.—HIS LITERARY EDUCATION.—HIS ADMISSION TO THE POLYTECHNIC SCHOOL.

Stephen Louis Malus, whose name will be perpetuated by an immortal discovery as long as the physical sciences shall be honoured among men, was born at Paris, on the 23rd of July, 1775, his parents being Anne-Louis Malus of Mitry, treasurer of France, and Louisa Charlotte Desbres.

His first studies were principally literary; he acquired a very sound knowledge of the authors who form the glory of Greek and Latin literature. Up to his latest years he continued to be able to recite, without hesitation, long passages of the Iliad, of Anacreon, Horace and Virgil. Like almost all scholars gifted with some facility of composition, he rashly devoted his youthful talents to productions of a kind really above his powers, and the difficulty of which one of our great poets so energetically characterises when he calls them "œuvres du démon." But he carried out his endeavours to an extent beyond what is usual. I have discovered among the papers of Malus, two cantos of an epic poem entitled *Thémelie, or the Foundation of France,* and two complete tragedies: one on the capture of Utica, and the death of Cato; the other recounting the dreadful catastrophes of the family of the Atrides, and entitled *Electra.* The fact that some beautiful verses and some interesting situations occur, would not hinder me from avowing that the youthful author had not as yet discovered his true vocation, were it not that the immense inequality which we observe between the *Hostile Brothers* and the *Andromache,* though both worthy of Racine, shows with what caution we ought to abstain from premature judgment.

Malus pushed forward with equal and distinguished success the study of letters, and of algebra and geometry. He went through the examination for the School of Engineers at Mèzières, in 1793, and was classed the same year as sub-lieutenant in the promotion in which General Bertrand held the first place. But the serious disorders of which the school of Mèzières was the theatre, having caused its suppression, Malus could not profit by his brevet of admission. He enrolled himself as a volunteer in the 15th Battalion of Paris, and proceeded to Dunkirk, where he took part in the manual labour of the wheelbarrow, as a common workman in the construction of the field fortifications with which that place was being surrounded. M. Lepère, engineer of roads and bridges, who was directing a part of these constructions, having remarked certain peculiar and unexpected arrangements in the manner in which the soldiers executed the excavations and raised the mounds, was desirous to learn the origin of these practices; they pointed out to him the man who had indicated these as the means best suited to attain the desired end with the least possible fatigue. A few moments' conversation showed the engineer that he had found in the humble labourer of the 15th Battalion of Paris a superior man; and he accordingly sent him to the "Ecole Polytechnique," which had just been founded.

Malus then was one of the first pupils of this celebrated institution. He soon gained the goodwill of Monge, who became his friend; indeed nothing less than such a warm friendship was necessary to preserve him from the misfortunes he would have incurred from his taking a part in the many political movements by which the capital was then agitated.

On quitting the school, Malus went to Metz, where he was received as a pupil sub-lieutenant of engineers the 20th February, 1796. He was named captain on the 19th June following; and was sent the next year to the army of the Sambre and Meuse, where he took an active and distinguished part in the actions in which that valiant army was engaged.

There has been recently found among the family papers, a small bound book, in which Malus, when captain of engineers, and employed in the army of the East, traced day by day an abridged narrative of all the events of which he had been an eyewitness, or in which he had taken a direct part. These memoranda, which I have read with the greatest interest, and in which our fellow labourer figures chiefly as a military man, seem to me to deserve a detailed analysis. I have resolved to lay it before you, were it only to prove once more, that profound knowledge and a scientific genius did not weaken either the zeal, the constancy, the courage, or the

spirit of enterprise, which ought to distinguish an officer of the highest military qualities.

After having read the following details, few would venture to estimate their own services above those which Malus, the man of science, rendered in his sphere.

EGYPTIAN CAMPAIGN.—EXTRACTS FROM THE MEMORANDA OF MALUS.

The events of the war led the Captain of Engineers Malus to the right bank of the Rhine. He remained eleven months in garrison in the learned city of Giessen: he was even on the point of contracting a marriage with the eldest daughter of the Chancellor of the university, Professor Koch, when the order came for him to proceed to Toulon, where he was to serve under Caffarelli in the left wing of the army, collected for an expedition of which scarcely any one knew the destination.

The 27th of Floreal*, we find him at Toulon, embarked on board L'Aquilon, a vessel of seventy-four guns, commanded by Thevenard, and making part of the advanced guard of the squadron. The 22nd Prairial † he took part in the attack, by assault, of the fortress of Malta, the defenders of which, he says, surrendered after having made much noise and done little mischief.

After a short sojourn in Malta, Malus, at the desire of General Desaix, commandant of the division which had arrived from Civita Vecchia, went on board the Courageux, in which that general was embarked. He remarks, "I had in all respects to congratulate myself on this change." The fleet quitted Malta the 3rd of Messidor‡, and we find Malus on the 13th of that month§ sailing all night in an undecked sloop in search of the General-in-Chief, to receive his orders as to the point at which the division of Desaix was to disembark.

On the 17th ‖ Malus was attached to the advanced guard of the invading army. The 21st ¶, in the evening, he encamped on the road from Ramanièh. At that time the corps of engineers had neither "materiel" nor troops. An officer of this service, isolated in the army, was often deprived of the commonest necessaries. We find an instance in the following description, which I quote from the memoranda: "Wanting a picquet to which to attach my horse, I tied him to my leg; I slept, and dreamt peaceably of the pleasures of Europe." On the 25th **, he took part in the glorious battle of Chebreys against the Mamelukes.

* May 16. † 10th June 1798. ‡ June 21. § July 1.
‖ July 5. ¶ July 9. ** July 13.

The 2nd Thermidor*, at the battle of the Pyramids, he was in one of the battalions formed in squares on the right wing beside General Desaix.

On the 4th †, in the morning, Captain Malus went with a detachment of carbineers into the island of Raouda, reconnoitring the right bank of the Nile to Mekias, and sent over to the left bank the boats which were necessary to enable the army to cross the river. The same evening he accompanied General Dupuis, who was charged with regulating the conditions of the capitulation of Cairo. On the 15th Thermidor ‡, he set out with the advanced guard of the army, which marched against Ibrahim Bey encamped at Belbeys, and took a very active part in the important combats which signalised this expedition; in which many military errors were committed.

Somewhat later, we find Malus accompanying General Regnier in a reconnoitre which had for its object the determination of the exact distance from Salchiéh to the sea. On his return he discovered the remarkable ruin of the ancient city of San, or Thamis. It was during this expedition that he learned the destruction of the French fleet in the naval battle of Aboukir; and we read without surprise in the memoranda that he re-entered Cairo fatigued, ill, and a prey to profound grief.

About the period of which we are speaking, General Bonaparte created the Institute of Egypt. Malus was one of its first members.

Some days afterwards, Malus received an order to join General Desaix in Upper Egypt. On his return to Cairo with the division of "the just Sultan," he was charged with the duty of making preparations for the fête of the 4th Vendémiaire §, in the square of Esbékiéh. "This was," he says, "a trifling distraction from the grief which had afflicted me for some time." On the 30th ||, and following days, Malus powerfully contributed to repress the insurrection which had arisen in Cairo: having arrested with his own hand, in the heat of the tumult, one of the insurgents, he found in his possession objects which he knew belonged to General Caffarelli, his immediate commander and friend; from these he believed that he had been killed; and it was not till after two days that he learned that Caffarelli had quitted his house before the Turkish revolters had pillaged it.

After the rebellion had been suppressed, Captain Malus commenced the establishment of a fort in the position whence during the insurrection they had connonaded the grand mosque. The

* July 20. † July 22. ‡ August 2.
§ September 25. || October 20.

construction of this fort occupied him a long time; it received the name of Dupuis. Afterwards he commanded at the reconnoitring of the communications of the Nile with the lake Menzaleh and with Salchièh. In this expedition the young officer made discoveries of great interest in respect to archæology, and the ancient geography of this part of Egypt.

On his return to Cairo, Captain Malus enjoyed some little leisure; by which he profited in order to examine in detail the "Well of Joseph," which he described as a masterpiece of perseverance and skill in construction. He went also to visit the colossal pyramids of Gizeh, in company with a man who might be truly called the colossus of our army from his height and his bravery, General Kléber.

When the army set out on the expedition to Syria, Malus, who was then occupied in reconnoitring the Delta, was attached to the division of General Kléber. We shall not follow him in the difficult route which our brave soldiers had to traverse almost without provisions or drinkable water: the details which we find on this subject in the memoranda only inspire the most painful reflections; we will merely say, that the young officer of engineers took a part with distinction in the siege of El-Harisch. We find him taking by assault, and with great intrepidity, an advanced post situated eighty metres from the place, — commanding in the trenches, and pushing the sap almost up to the foot of the breach, when the enemy offered to capitulate. The young officer denounced in energetic terms the breach of faith of which our generals were guilty in regard to the prisoners, in forcing them to enlist among our soldiers.

Malus relates the march of the army advancing into Syria. It first took the infection of the plague in the town of Gaza abandoned by the enemy: its divisions arrived at length before Jaffa and invested that town, of which it raised the siege. The operations were conducted in a way which was not conformable to the rules of the science originally laid down by Vauban. Our young officer recounts that the breaching battery, being supported by armed positions on too small a scale, was surprised in the night by a sortie of the troops from the town. The heads of our soldiers carried into Jaffa were paid for by their weight in gold. The head of Malus, however, did not figure in the number of these bloody trophies, for the sole reason that at the moment of the silent invasion of the battery by the Turks he was asleep in one of the angles of the entrenchments. The breach having been opened, and the garrison not having answered to the summons made them, the troops advanced to the assault to the sound of the bands of all the regiments. Here I will no longer abridge, but copy: —

"The enemy was overthrown, discouraged, and retired, after a sharp firing of musquetry from the houses and forts of the city: they kept their ground, however, at some points, and continued their fire for an hour. During this time the soldiers, scattered through all parts, killed men, women, children, old persons, Christians, and Turks;—everything that bore the human form was the victim of their fury.

"The tumult of carnage, the broken doors, the houses shaken by the noise of the firing and of arms, the cries of the women, the father and child overthrown one on the other, the violated daughter on the corpse of her mother; the smoke of dead bodies burned in their garments which had been set on fire, the smell of blood, the groans of the wounded, the cries of the conquerors disputing together over the spoils of their expiring prey, infuriated soldiers responding to the cries of despair by exclamations of rage and redoubled blows; lastly, men satiated with blood and gold, falling down in mere weariness on the heaps of corpses;—such was the spectacle which this unfortunate city presented until night."

This forcible passage of the manuscript of Malus is the faithful picture of what happens in every town taken by storm, even when the assailants belong to the most humane civilised army in the world. When historians know how to place themselves in a more elevated sphere, to free themselves from routine, and to follow in the opinions they express the eternal rules of justice and humanity, while they praise the indomitable courage of soldiers who will brave death in obedience to discipline, they will accord a deeper sympathy to the men who to preserve their nationality consent to expose themselves to scenes of massacre and bloodshed such as those which the narrative of Malus has revealed in all their horrors: their condemnation will be reserved for those who provoke these impious wars, which have no other motive than personal ambition, and the desire for a vain and false glory.

When the army set out for the attack on the town of St. John d'Acré, Malus received an order to remain at Jaffa with General Grezieux. There were left with him only 150 efficient men: the town contained more than 300 wounded and 400 infected with the plague. Malus was charged with the arrangements necessary to be made in the Greek Convent, in order to establish there those suffering with the plague. For ten days successively he passed all his mornings in the infected air of this receptacle of corruption. Thus our celebrated painter Gros might have legitimately placed the portrait of Malus among the figures in that admirable picture for which modern art is indebted to him, in the

place of some of those conventionally introduced there, who never really penetrated into the halls then choked up with the dying and dead.

The eleventh day, Malus felt himself infected with the terrible disease which decimated our army. From this moment I will allow him to speak for himself; science may perhaps derive some advantage from the details which I transcribe: —

"A burning fever, and violent pains in the head, forced me to seek repose: a continued dysentery was added; and one by one the symptoms of plague showed themselves. About the same time General Grezieux died. Half of the garrison had already been struck: thirty soldiers fell victims daily; Brinquier, who had taken my place in superintending the hospital, was seized on the fourth day, and died forty-eight hours afterwards. At this period characteristic bubo showed itself on my right groin. I had all along up to this time entertained hope that my disease might not be the plague; the number of days I had lived since the first attack seemed to indicate it: but since the bubo appeared, and the pains at the heart were redoubled, I could no longer feel any doubt; I resigned myself to my fate. I sent to Francisqui, who was with the wounded General Damas, the articles which I wished to leave to my relations and friends. I ought to remark that Francisqui was the sole one of my comrades who had not abandoned me, and who, in order to tranquillise me, had not hesitated to come near me; on the day of his departure he carried his devotion to such an extent as to embrace me, though he was then certain that I was infected.

"Only one man in twelve escaped. St. Simon arrived in Egypt and came to see me: he was then in perfect health, in two days afterwards he was dead. The siege of Acre was protracted, the sick fell back on Jaffa and increased the numbers of the dying: besides this, the plague was in every house of the town where there still were any inhabitants. The refugees of Ramlè, who came to Jaffa to place themselves under our protection, perished nearly to a man. The Convent of the Capuchins, which was placed in quarantine, could not escape the contagion: the greater part of the monks died. All the Frank families perished except two men and one woman.

"I no longer knew a single individual among those now at Jaffa. I had lost successively my friends, my acquaintance, and my servants; there only remained my French servant, who attended me with constancy during my illness, and he died at my side the 24th Germinal.* I was now alone, without strength,

* April 12. 1799.

without help, without friends: I was so exhausted by the dysentery and the continual suppurations, that my head became extraordinarily weakened: the fever, which redoubled its intensity at night, often made me delirious and agitated me terribly. Two men of the corps of sappers undertook the care of me, and they perished one after the other.

"At length on the 2nd Floreal * I was put on board L'Etoile, which was setting sail for Egypt and whose captain had the plague: he died the night of our arrival at Damietta. The sea air produced a sudden effect on me: it seemed to me as if I were relieved from suffocation. After the first day I almost began to feel some wish for food, I was nevertheless very feeble. Contrary winds kept us several days out at sea: this delay produced a very marked amendment in my health: my strength revived, the crust of the bubo fell off: my appetite was restored.

"On the 7th Floreal † we came to anchor before the Bogaz of Damietta: on the 8th ‡ we entered the Nile and the vessel was put under quarantine."

If any one would wish to know how our institutions, when entrusted to persons destitute of humanity, add fresh sufferings to those of natural afflictions, let him continue with us the transcription of Malus's harrowing recital.

"The 10th Germinal § I disembarked and was conducted to the lazaretto of Lesbièh, where were collected those suffering from the plague from Damietta as well as those arrived from Syria. They placed also with me several passengers who had no symptom of the disease, but who in due course took the infection in the lazaretto and died every one of them. These numerous deaths delayed the period of my enlargement. It was rare that any one got out of this infernal prison who had once had the misfortune to enter it: hardly would they condescend to succour the unhappy persons who came to spend their last hours there. I have often seen them die with rage demanding water of the barbarians who pretended that they did not understand them, or would answer, 'It is not worth while.' Greedy gravediggers robbed the dying persons before they had yielded their last breath; these unworthy agents of the sanitary commission were the only medical attendants, the only guardians allowed to the sick. Hardly had their victims ceased to live when they carried them over to the opposite shore, where they abandoned them to the dogs and birds of prey. Sometimes they covered them with a little sand; but the wind soon exposed the bodies

* April 21. † April 26. ‡ April 27.

§ Probably a mistake for Floreal, April 30.

naked, and the cemetery presented the hideous spectacle of a field of battle. One wretched woman, of whom I had taken care because she was absolutely deserted, begged of me the evening of her death to give a piastre to the grave-diggers, that she might be preserved from becoming a prey to the jackals. I fulfilled her wish, and caused them to bury her at the extremity of the plain where the dead were deposited.

"I had been already a month in this abominable abode, when Cazola obtained for me the privilege of being put in quarantine in a separate lodging. My solitude appeared to me delicious, because I had quitted the society of the dying. I succeeded in re-establishing my health, and in the beginning of Messidor *, I received definitively my liberty, which followed the sacrifice of all my property."

How heartily must we not congratulate ourselves that Malus escaped, in so unhoped-for a way, from the terrible stroke which had mowed down so many victims! If he had fallen under it, the beautiful branch of optical science, of which he planted the first signal after his return to France, perhaps would not have been created, and the admirable progress which the science has made would not have been reckoned among the most striking claims to the admiration of posterity of which the 19th century may boast. Some time after this, Malus was ordered to proceed to Cathièh, where he established himself. The delights of this advanced post, where General Le Clerc commanded, are described *con amore* by him who had just escaped the frightful disease, and the dangers not less dreadful of the lazaretto of Lesbièh.

"We encamped," he says, "in huts whose walls and roofs were composed of palm leaves interwoven: we were lodged like Arabs; I had close to my cabin a small enclosure containing my horses, camels, and asses; an aviary full of fowls, geese, and ducks, a pen for my two sheep, another for a boar; houses for my pigeons, and my goat enjoyed its liberty. It was in a great measure in this society that I passed three months of my sojourn in Egypt which were to me particularly agreeable. A perfect tranquillity, peaceable enjoyments, and waiting for an enemy whom we calculated on conquering, hindered us from wishing for conveniences of which we were deprived."

Malus here does not say all: at Cathièh he composed a memoir on light, of which we shall have occasion to speak presently. If it should happen that in analysing this work, we should find therein some results which may, or which ought to be, contested, we may remark that it was composed half a century ago, and that the author was in a position truly exceptional when he was engaged in it.

* June 19=Messidor 1.

I find mentioned in the memoranda that in a reconnoissance which he made with a detachment of dromedaries of which he had the command, Malus encountered a caravan, attacked it, dispersed it, and obtained a great number of camels, and a quantity of provisions.

On quitting Cathièh Malus went to Cairo, where he received from Kléber (October 21. 1799,) the brevet rank of Chief of Battalion, the just recompense of such active services and so much courage displayed by the young captain, ever since the first disembarkation of the French army in Egypt. The commandant Malus having learnt at Cairo that a disembarkation of Turks was preparing near Damietta, hastened thither: where, when he arrived the 8th Brumaire *, he found the enemy already fortified. The next day but one, after having been in the trenches during the morning, he joined, as a private foot soldier, the troops who charged the Osmanlis with the bayonet, and precipitated them into the sea.

On the 20th Frimaire † Malus received the command of the position at Lesbièh, where he had destroyed the walls when this fortress was in the hands of the Turks, and which he had rebuilt since it had fallen into the power of the French. On the 22nd ‡ the plague made its appearance at Lesbièh in six different quarters: the commandant Malus, from his long experience, applied means of preventing its development and propagation; nevertheless it made many victims till the 28th Pluviose.§ On the 29th|| the position of Lesbièh was surrendered to the Osmanlis in virtue of the convention of El Harisch. Malus arrived at Cairo the 25th Ventose ¶, and on the 28th ** learned the rupture of the capitulation of El Harisch by Lord Keith. The same day, at two o'clock in the afternoon, appeared the proclamation of Kléber, which ended with these celebrated and prophetic words: "The army will respond to this disloyal proceeding, and to the demand to lay down their arms, by new victories." The army was in fact on its march on the next day to fight the forces of the Grand Vizier. Malus, attached to the division of General Friant, personally took part in the immortal battle of Heliopolis, when 11,000 men triumphed over more than 60,000.

The day after the victory a particular circumstance which I find related in the memoranda had some unfortunate consequences. "On the 30th ††, at two o'clock in the morning," Malus says, "the army commenced its march for Belbeys, where we reckoned on finding the Turkish army collected. I went with the division of

* October 29. † December 10. ‡ December 12.
§ February 26. 1800. || February 27. ¶ March 15.
** March 18. †† March 20.

Friant. After an hour's march I suspected that we were losing our way in the desert. As the night was very dark we had lost the ordinary tracks. I represented the matter to the general, who listened to me for a moment, but other persons brought forward opposite opinions with so much assurance, that the march was continued. One hour and a half afterwards, we were taking a direction exactly towards the point whence we had started. This I perceived from the position of the pole star, which we had at starting behind us. This time I was listened to, and I led back the division on the right route. This mistake nevertheless caused us much delay, and the other divisions were obliged to wait for us at one league distance from Belbeys."

We see on what little circumstances the great events of war often depend. If there had been in the division of Friant only an ordinary small compass of a few millimetres in diameter, like those which are hung among the trinkets to watches, or even if self-conceited officers had not obtained a preference for their opinions over that of Malus, the divisions of our army would have been reunited much sooner; and that of the Grand Vizier would have experienced near Belbeys very considerable losses.

Malus, now attached to the division of General Regnier, took part in the expedition which, after several serious affairs, drove back the Ottoman army across the desert. Afterwards he returned to Cairo, then in a state of revolt excited by the Mamelukes, who on the day of the battle of Heliopolis fell back on the great city. We see at once the nature of the service of an officer of engineers in such an attack as that on Cairo, where he was obliged, in order to take the barricades, to turn them by passing through the interior of the houses. After the complete surrender of Cairo, Malus was quartered at Gizeh, when on the 25 Prairial *, General Kléber was assassinated in his garden at Cairo by a Turk arrived from Syria.

We will here terminate the long extract from the memoranda of Malus. It would be too painful to us to follow the well-founded, but very bitter criticisms which he directs against General Ménou. A single trait will suffice to show his opinion of the former Commander-in-Chief of the army of the East. "Kleber," says Malus, "was assassinated on the 24th Prairial: some days afterwards General Ménou, in attacking the honour of the deceased General Kléber, has assassinated him over again."

In going over the memoranda, which, amid the chances of war, might very probably fall into the hands of indiscreet persons, friends or enemies, I remarked that Malus indicates very exactly

* June 13.

the date at which he received letters from his father, his uncle, &c. As to letters from Giessen (and we easily guess whose hand wrote them) he gives no indication or trace. I notice this extreme delicacy for the instruction of ill-informed, or malevolent persons, who believe sentiments of the kind referred to incompatible with geometrical studies.

MARRIAGE OF MALUS.—HIS MILITARY CAREER.

Malus quitted Egypt and made the voyage on board the Castor, an English transport ship, according to the arrangement made between General Ménou, Commander-in-Chief of our army, and the hostile generals. He arrived at Marseilles the 1st Oct. 1801, and was immediately put into quarantine. After the pestiferous scenes of Jaffa, of Damietta, and Lesbièh, he must have found the lazaretto in which he was now confined a place of luxury. As soon as he was set at liberty he repaired to Paris. After a short visit to his relations, bound by his sentiments even more than by his promise, he hastened to Giessen, where he once more joined Mademoiselle Wilhelmine Louise Koch, affianced to him for four years, and was married to her. This union completed his happiness: we shall soon have to relate the rare proof of devotion which Madame Malus gave the husband of her choice during the afflicting illness which took him from her and from the sciences.

The subsequent military career of Malus may be stated in a few lines. In 1802-3 he was employed at Lille. We find him in 1804 at Antwerp, planning measures, according to the orders of Napoleon, for completing the naval establishment of this city, and extending its lines of fortification. In this elaborate work, the account of which is preserved in the depôt of fortifications, accompanied by eleven sheets of drawings, the author treats analytically, but without neglecting the arithmetical applications, two questions of mechanics, which, under the circumstances and in that locality, possess a great importance; viz., 1. The amount which ought to be deducted for the weight of men marching in a tread-wheel, to move the inclined twisted pipes, or Archimedean hydraulic screws, used in draining: 2. the employment, for the same purpose, of the force of wind, acting on wind-mills having horizontal sails disposed in such a way as to turn always in the same direction. In 1805, Malus was attached to the Army of the North. In 1806-7-8, he was sub-director of the fortifications of Strasbourg. In this capacity he presided over the reconstruction of the fortifications of Kehl, and made some very judicious remarks on the form of the revetments *, and applied an exact

* The masonry encasing and supporting the earthworks in a fortification.

analysis to the determination of their thickness. In 1809 he was recalled to Paris. He became major of engineers in 1810. The archives of the Committee of Arms prove that the inspectors-general often consulted him with much advantage on the merit of works submitted to them.

MEMOIR ON LIGHT. — COMPOSED IN EGYPT.

We have henceforth to occupy ourselves only with the life of Malus as a physicist and member of the Academy; without departure from this view, I may say a few words on the optical memoir which he composed in the hut at Lesbièh.

The author announces clearly, in the first part of the MS. memoir which I have before me, the object which he proposes: this is to prove that light is not a simple substance; that its constituent principles are caloric and oxygen, in a particular state of combination. To establish this theory, he cites numerous facts furnished by chemistry, which prove that he was perfectly initiated, not only in the general principles, but even in the details, of this science. It must be observed, however, that all the deductions of Malus, even the most plausible, at the present day, can be subverted by a single word: it suffices to cite, in contradiction to all the phenomena which our friend alleges, the instance of the light which is engendered in a vacuum, by the aid of the voltaic current, passed through simple substances, such as carbon, platinum, &c.

In the second part of the memoir Malus seeks to establish that the different natures of various lights only differ from each other in the greater or less proportion of caloric which they contain. The red light would thus be the most heating, the violet the least so, which agrees with experiment. According to a singular opinion professed by the author, all rays, if possessing a certain high *intensity*, ought to produce the sensation of whiteness.*

* The "singular" opinion here ascribed to Malus is perhaps not altogether without foundation, at least in some cases. It is certain that while the prismatic spectrum of the white light of the clouds presents a clear *yellow* and *green* portion, that same portion, when the direct rays of the sun are substituted, appears to the eye intensely brilliant and *white*. And it is far from certain that in some other experiments, which have been the occasion of some little controversy and where the *colour* of certain parts of the spectrum has appeared to undergo a change, the *intensity* of the light reaching the eye may not be concerned. In fact, the *sensation* of colour is one so entirely dependent on unknown physiological causes, that we can hardly venture to predict what the result may be on different individual eyes, though all the optical conditions may be precisely the same. It may not be altogether without a bearing on this subject, to remark the extremely contradictory statements made by different observers as to the *colour* of intensely brilliant meteors. — *Translator.*

The third part of the work is devoted to mechanical consequences which result by analysis from the suppositions explained in the first two sections. It may suffice to say, that the author finds, like all the partisans of the system of emission, that the velocity of light ought to be greater in water than in air: every one therefore will see how superfluous it would be now to go into a discussion of the details of such a subject.

The memoir of which I am speaking was destined for the Institute of Egypt. I find in fact, in a letter from Malus to Lancret, the following passage: —

"I send you, my dear Lancret, the work of which I have already spoken: mark out for me those things in it which any one might call repetitions of what has been already said, or which are useless; if, after this expurgation, it should be reduced to zero, we will put it aside, and there will be no more question about it."

It is just to remark, after the critique from which I could not abstain when I considered that my task was not that of a panegyrist but of a biographer aiming at the truth, that the third part of the memoir was written before the publication of the fourth volume of the *Mécanique Céleste*, in which the same subject is treated with the greatest care. I would add that no army in the world ever before counted in its ranks an officer who occupied himself in the spare hours of advanced posts with researches so complete and so profound. The truth of this remark is not affected by the recollection which it brings up of the expedition of Alexander. It is true, men of science, at the recommendation of Aristotle, then accompanied the great general; but their mission was solely to collect the scientific achievements of the conquered nations, and not to make advances in the sciences by their own labours. This difference, altogether in favour of the French army, deserves, I think, to be here noticed.*

I see by a letter of Lancret, of the 14th Vendémiaire † an. IX., that Malus was occupied theoretically with that most important meteorological question, the distribution of heat in different climates. I have never been able to find what has become of this work.

TREATISE ON ANALYTICAL OPTICS.

On the 20th April, 1807, Malus presented to the first class of

* If this comparison were worth carrying out, the author might have added that the men of science in Alexander's expedition were not *officers of the army* charged at the same time with onerous and hazardous duties, but leisurely investigators, having no other occupation. — *Translator.*

† October 5. 1800.

the Institute, a treatise on analytical optics, in which he treats of rays of light by geometry of three dimensions.

The choice of academicians to whose examination the work was entrusted, sufficiently indicates the reputation which the author had already acquired. These commissioners were Lagrange, Laplace, Monge and Lacroix. The report of this distinguished commission was presented by Lacroix, and bears date the 19th October, 1807.

The author of the memoir examines the nature and relative position of the surfaces formed by straight lines successively intersecting one another according to given laws. After having deduced from his researches some general theorems, of a very remarkable kind, he proceeds to make an application of them to the case of rays of light proceeding in similar directions, either by reflexion or by refraction. He thus generalizes the theory of plane caustics, formerly broached by Tschirnhausen. Among the curious results which he deduces from his formulas, we will merely quote the following:—

"Reflexion and refraction furnish sometimes optical images which are erect in one of their dimensions and inverted in the other."

The report, for which I will not presume to substitute my personal opinion, concludes in these terms:—

"To apply thus, without any limitation on its generality, calculation to phenomena;—to deduce, from a single consideration of a very general kind, all the solutions which before were only obtained from particular considerations,—is truly to write a treatise on analytical optics, which concentrating the whole science in a single point of view, cannot but contribute to the extension of its domain."

The Academy decided (which is the highest degree of approbation it can bestow) that the memoir of Malus should be printed in the *Recueil des Savants Etrangers.**

* Malus's analytical theory contained in his *Traité d'Optique*, is prefixed to his prize memoir on Double Refraction, Paris, 1810.

The ordinary deviations by reflexion or refraction which rays undergo on impinging on given surfaces, may be investigated in all the simpler cases by means of elementary geometrical constructions, leading to the theory of foci, caustics, &c. But more general investigations of the same kind have been pursued by considering the algebraic equations of rays undergoing such deviations. This higher generalisation leads to, and includes, the same results. An excellent discussion of the subject treated in this point of view will be found in Dr. Lloyd's *Treatise on Light and Vision.* It is a still higher generalisation of this kind which was followed out by Malus. The reader who is desirous of seeing a condensed abstract of the leading mathematical principles involved, is referred to a brief but luminous summary drawn up by the Rev. A. Neate, M.A., and

MEMOIR ON THE REFRACTIVE POWER OF OPAQUE BODIES.

On the 16th November, 1807, Malus presented to the Academy a memoir in which he treats a point of optics of great importance, a question, in fact, involving no less than the grounds for a decision between the claims of the two rival theories of light.

The celebrated physicist Wollaston, some years before, had proposed a method by means of which to deduce the refractive power of all substances whether transparent or opaque. This method rests on the determination of the angle under which these substances applied immediately in contact with one of the surfaces of a prism of glass, through which we look at them, begin to cease to be visible.

Now according to the theory of reflexion * expounded in the

inserted in Professor Powell's *Elementary Treatise on Optics*, p. 71., Oxford, 1833. But the entire subject has been treated by a far higher analysis with extreme generality, and by a new and powerful principle of his own, by Sir W. R. Hamilton, in his essay on the *Theory of Systems of Rays*. Mem. of R. Irish Academy, vols. xv. and xvi., and Supplement, vol. xviii.—*Translator*.

* To render what follows intelligible, many readers may find it perhaps desirable if we here explain, very briefly, the view of *ordinary reflexion* and *refraction* of light as explained respectively by the *emission* and the *wave* theories.

On the former a molecule of light resembles an elastic body, which if projected obliquely against a hard plane surface, by the principles of mechanics rebounds at *an angle equal to* that at which it impinged.

In refraction the investigation is more difficult: a molecule of light is here supposed to enter, projected with great velocity, among the molecules of the refracting transparent medium which are at such relative distances as to allow it freely to pass among them; but at its first entry among them it is of course *attracted* by them; it then becomes a problem of dynamics, requiring the aid of the higher mathematics, to determine what will be the path which it will pursue under their influence. In general it is clear, that under these united attractions urging it on, its velocity will be *accelerated*: but to go into the complete solution, would be beyond the limits of a note. It was fully investigated by Newton (*Principia*, lib. i. sect. xiv. prop. 94.), where he demonstrates that on these principles the deviation of the refracted ray will follow the law that the *sines* of the angles of incidence and refraction are in a *constant ratio*.

Similar investigations have been pursued by Laplace, more especially with regard to atmospherical refraction, the atmosphere being supposed to consist of strata of different densities. (*Méc. Céleste*, vol. iv. liv. x. ch. i. 2, 3.)

On the *wave* hypothesis, the explanation admits of a very simple kind of illustration.

A set of waves propagated circularly from any source, when they get to a considerable distance, may be regarded as proceeding in parallel planes. In all cases, the portions of circles or spheres which are their true form have a common tangent which marks what is called the "front" of the wave.

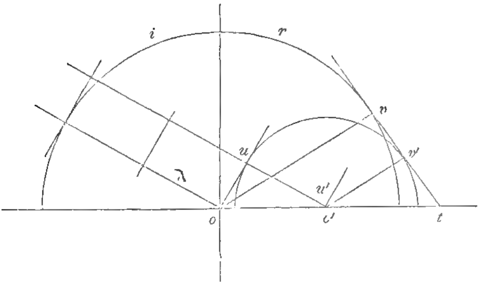
But whenever waves encounter any kind of obstacle, or *enter any new medium*, then, from and round each point of such encounter, a new set of spherical

10th book of the *Mécanique Céleste,* and founded on the corpuscular hypothesis, the formulas would be different for opaque and

waves begins to spread. In *denser* media these new waves spread *more slowly* than in *rarer*, but when the obstacle is still surrounded by the same medium, then the velocity is unaltered.

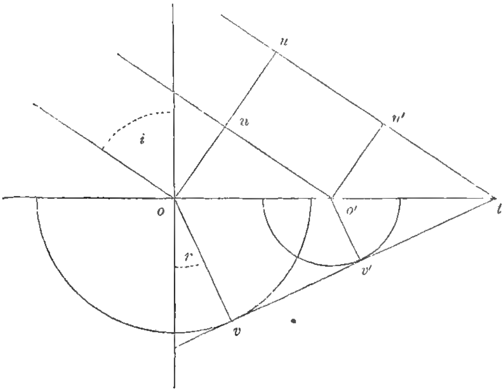
On these principles the ordinary laws of reflexion and refraction are proved on the theory of waves.

In reflexion, if parallel waves $u\ u'$ follow at equal intervals λ, u impinging on



the surface at o, will cause a new circular wave to spread backwards from that point as a centre; when the next wave u' impinges at o', it will do the same, and so on in succession. But when the wave from o' has spread to a radius $=\lambda$, that from o will have spread to a radius $=2\lambda$, and so on. Hence to these cotemporaneous circular waves drawing a common tangent $v\ v'\ t$ this will be the front of the reflected waves, and the radii to the points of contact $o\ v$, $o'\ v'$, will give the *inclination* of the *reflected* rays, which is easily seen to be *equal* to that of the *incident*, since $o'\ v' = o'\ u = \lambda$, and $o\ v = 2o'\ v'$, whence $o\ o' = o'\ t$, and the triangles upon these equal bases being right-angled, the angle $v\ t\ o = u\ o\ o'$, or the angle of *incidence*, is equal to that of *reflexion.*

For refraction; by an analogous construction, the circles which spread in the

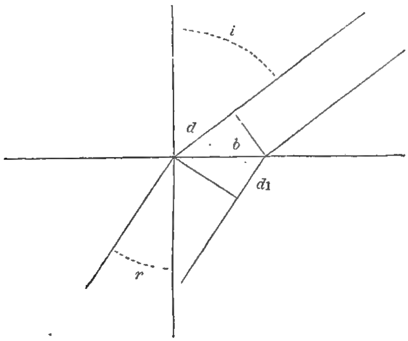


denser medium are smaller than those in the first, the radii being *diminished* in the ratio of the *velocities* or inversely as the densities. Thus when the new wave originating at o' has spread to v', that from o will have spread to *double*

for transparent bodies. It is on this point then, they would say, that Wollaston was deceived. The object which Malus proposed in his memoir was to submit this point to a decisive experimental test. He chose a substance, bees' wax, whose refractive power could be measured in the transparent state, and in the opaque state by the method of Wollaston. He applied to the angles of disappearance corresponding to these two conditions, and sufficiently different one from the other, the formulas of the *Mécanique*

the same radius at v. The common tangent or front of the refracted waves will be inclined at an angle $o\,t\,v$, which is easily determined by drawing the parallel through t of the incident light, whence we have (i and r being the angles of incidence and refraction) $u\,t = o\,t$ sin. i, and $o\,v = o\,t$ sin. r; but $o\,v$ and $u\,t$ being the radii of waves in the two media, are in the constant ratio of the densities $= \mu$; hence sin. $i = \mu$ sin. r, which is the experimental law of refraction.

The law of refraction may also be more briefly deduced thus: — taking the fronts of the incident and refracted rays perpendicular to their directions, their



inclinations will be determined by the relative velocities with which those fronts advance; and while the incident front has advanced through a space d, that of the refracted will have advanced through d_1 proportional to their velocities; or,

$$\frac{d}{d_1} = \frac{v}{v_1} = \mu.$$

But geometrically for any breadth,

$$d = b \sin. i \qquad d_1 = b \sin. r,$$

Hence,

$$\mu = \frac{\sin. i}{\sin. r},$$

which is the law of refraction.

This method, though in a less concise form, is given by Mr. Power (*On Absorption of Rays, &c., Philos. Trans.*, 1854, pt. i.), who nevertheless calls in question the principle of the assumption that the *front* of the rays is strictly perpendicular to their direction, and proposes a more general view: from which without any assumption as to the nature or law of refraction, he shows that the formula of the sines is directly deducible from his analysis. Objections, however, have been raised against his reasoning. — *Translator.*

Céleste, and he found there would result refractive powers perfectly identical. This identity of the refractive powers of wax, when transparent and when opaque, which seemed to be a necessary result, appeared both to the author, to Laplace, and to all the mathematicians and physicists of the Emission School in Europe, to afford a mathematical proof of the truth of the emission theory. It is assuredly a singular thing that there should be this perfect identity of refractive powers calculated from angles of disappearance differing from each other, and according to formulas very dissimilar between themselves.

But what proof was there that the refractive powers ought to be identical? Ought we to suppose that the change from the solid to the fluid state in any substance would be without influence on its refractive power? Might we not cite cases in which heat modifies the refractive power of bodies independently of their density? Again, were the temperature of the wax and its density well ascertained at the moment of the experiment such as Malus was obliged to make it? Besides, would it be strange to suppose that within those limits where the action of bodies on light operates there are no substances truly opaque!

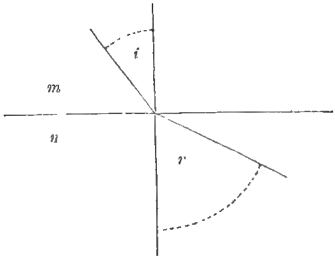
Now that the system of emission is overthrown without hope of restoration, I endeavour to recall all the circumstances by which Malus might possibly have been misled. But, for my own part, I feel sure that I do not deceive myself in affirming that the memoir of which we are speaking offers a new proof of the mathematical spirit and experimental talent which Malus possessed in so high a degree. We ought only to regret that the conclusions in the report were so explicit that they represented the atomic theory of light as completely established; and that such a decision, emanating from individuals so competent as Laplace, Haüy, and Gay-Lussac, may perhaps have contributed to alienate our illustrious associate from that experimental path which Fresnel a few years afterwards showed to be so astonishingly fruitful in results.*

* In the remarks here made by Arago on Malus's investigation of the refractive powers of solid and liquid wax, there appears some little obscurity of statement, and a degree of importance attached to the result as decisive between the rival theories, which it does not appear to deserve.

Perhaps for the general reader a few words explanatory of the method may be necessary, in order to see the general bearing of the case.

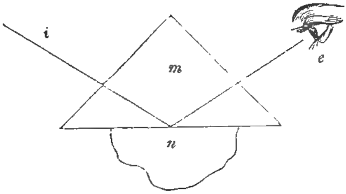
When a ray passes out of a denser medium m into a rarer n, the angle of refraction r will be greater than that of incidence i, according to the well-known law of the sines, which here becomes $\sin. r = \mu \sin. i$. But μ being constant for the same two substances, there is a certain limit to i when $\sin r = 1$ or $r = 90°$ or $\sin. i = \frac{1}{\mu}$ that is, the refracted ray coincides with the bounding surface of the media, or it ceases to be refracted: and if i exceed this value, $\sin. r$ would be greater

than unity, which is impossible, *or the ray cannot emerge* from the denser mediur but must remain wholly within it. This alone, however, does not prove that i



will be reflected. Experiment, however, shows that it is, and the precise angl i at which this begins to take place, or when $\sin i = \frac{1}{\mu}$ for any *pair* of media, ca be easily and accurately determined; thus μ is found for that *pair* of substance but μ is the compound ratio of the separate refractive powers of each out o vacuum or air; if, therefore, one of these is known, the other is deduced.

On this principle Dr. Wollaston's method was founded (*Phil. Trans.* 1802) Any substance n, of less refractive power than glass in optical contact with th base of a glass prism m, can be seen by an eye at e at any incidence *within* th



limit just mentioned, or while the ray i entering the other side of the prisr and impinging on its base, is incapable of being refracted out at the base, an therefore reflected from within; but as soon as this limit is exceeded, or the ra is refracted out at the base, then n ceases to be visible at e. The exact inci dence or "critical angle" at which this takes place, is measured by an appro priate apparatus, and the refractive index for n deduced, that of the prism bein known, a series of substances being applied in succession, whether transparen or opaque, Dr. Wollaston in this way determined their refractive indices. A the different primary rays have indices a little differing, and which are greate for red light, Dr. Young remarked that the *limit* thus found applies in strictne to the *extreme red ray*.

In this way Dr. Wollaston found the refractive indices as follows: —

White wax,	boiling - - - -	1·542
Ditto	cold - - - - -	1·535

In the same way Malus found

Wax at 14° Reaum. (=63° Fahr.)	- - -	1·5123
Ditto	melting - - - -	1·4503
Ditto	boiling - - - -	1·4416

(These numbers are all lower than the former, probably from a different so of wax being used.)

Dr. Wollaston, in applying the simple calculation above indicated to the ob served angles, did not question the very natural assumption, that the same fo

MALUS GAINS THE PRIZE PROPOSED BY THE ACADEMY FOR A MATHEMATICAL THEORY OF DOUBLE REFRACTION.

On the 4th January, 1808, the Academy proposed, as the subject for a prize in physical science to be decided in 1810, the following question: —

"To give a mathematical theory, confirmed by experiment, of the double refraction which light undergoes in passing through different crystallised bodies."

The memoir of Malus received the prize. Doubtless fearing lest he should be forestalled by some of the competitors, in the discovery of the singular properties of light which he had observed, this eminent physicist communicated the most essential parts of his researches to the Academy on the 12th December, 1808, without waiting for the period at which, according to the programme, the competition was to be closed. It is then to the end of the year 1808 that the immortal discoveries belong of which I proceed immediately to give you an analysis. The commission appointed to judge of the competitors was composed of Lagrange, Haüy, Gay-Lussac, and Biot. The report was presented by Lagrange, and thus nothing was wanting duly to signalise the important discovery of Malus.

DISCOVERY OF POLARIZATION BY REFLECTION.

We must go back to Erasmus Bartholimus to find the first observations relative to the existence of double refraction in Ice-

mulas would apply to the observed angles equally, whether the substance was opaque or transparent, solid or fluid.

Laplace, in a theoretical investigation founded on certain considerations derived from the molecular theory, framed his formulas on the assumption that the conditions were different for opaque and for transparent bodies, and even for the same substances in the two states respectively. The question at issue was the truth of this assumption, though it must be confessed that little appears in the tenth book of the *Méc. Céleste* by which this conclusion can be considered as established.

Malus observed by Wollaston's method the *angles* at which the disappearance took place in wax, solid and in fusion. These *angles* were different; and calculated in the usual way, the *indices* of refraction resulted different also (as seen in the above tabular view).

The same observed *angles*, however, calculated by Laplace's formula gave the resulting *index* the *same* in both cases.

Now Laplace, Malus, and the emissionists considered the identity of refractive power thus resulting to be a *necessary* truth — why so, we do not see; it is obviously, at best, a mere consequence of the *assumption* made at the first. The result is no proof of its truth, and decides nothing either way. Arago's laboured remarks therefore seem superfluous. — *Translator.*

land spar, also called calc spar, or rhomboidal carbonate of lime. Huyghens had occupied himself with the study of these phenomena, and pointed out a geometrical construction of a very simple and elegant kind by which we can determine, in all directions and at all incidences, the position of the extraordinary ray relative to the ray properly called the ordinary ray, whose position is determined by the well-known law of the sines, made known by Descartes. Huyghens arrived at the discovery of this construction by means of an ellipsoid, which, as he tells his readers, he derived from considerations borrowed from the theory of waves.

The reporter of the Academy on Malus's memoir of the 12th December, 1808, entitled *Memoir on a Property of Light reflected by transparent Bodies,* who was no other than Laplace, wished that Huyghens had been contented to have given his law as the result of experience only. But I may be permitted to ask, Is not the hatred of theory carried too far when it leads to the suggestion of dissimulation or the want of sincerity?

Newton contended for substituting other rules instead of that of Huyghens; but these have not been found conformable to facts.

Among modern observers Wollaston was the first who established the truth of the principles laid down by the Dutch philosopher. To make this verification he availed himself of the ingenious method by which he found the index of refraction by means of total reflexion. It appears that in 1808 these verifications had not appeared sufficient to the physicists of the Academy of Sciences since they proposed the question as the subject of a prize for experimenters. However this may have been, Malus translated the construction of Huyghens into analytical formulas: he compared the deviation of the extraordinary rays deduced from these formulas with the numbers resulting from very accurate observations, and the accordance was in all cases very perfect. Thus the geometrical conception of Huyghens was found to be completely established, although originally the author was led to it by theoretical views.

A ray of light divides itself into two rays which are of exactly the same intensity whatever be the position of the crystal which it traverses, and in which the division into two is produced. But the case is different when the rays pass out of one crystal and are received into, and analysed by, a second crystal exactly similar. If this second crystal is situated relatively to the first in such a way that the corresponding faces are respectively parallel to each other, the ordinary ray in traversing it only undergoes the ordinary refraction, and the extraordinary ray also remains exclusively an extraordinary ray. The natural light then in traversing the first crystal has thus changed its nature. In fact, if, in becom

ing double, it had preserved its original properties, the ordinary ray and the extraordinary would *each* have been divided into two rays in traversing the second crystal. At emergence from the second crystal we should have had four images instead of two. The first idea which occurs to the mind would be that the natural light is composed of parts which are susceptible, some of them undergoing the ordinary, some the extraordinary, refraction, and an equal number of each. But this hypothesis is radically subverted by a very simple experiment.

If we cause the second crystal to turn through one-fourth of a revolution round itself, retaining the parallelism of its upper and under surfaces to those of the first, the ordinary ray will now become extraordinary, and the extraordinary will now undergo only the ordinary refraction.*

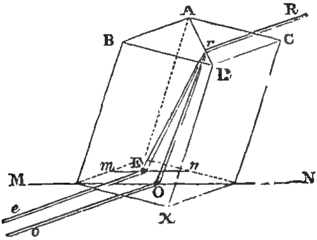
* The subject of double refraction, of which the most characteristic results are here stated by the author, is one which is rarely made intelligible to a general reader by a mere cursory description, and without going into some detail of the successive changes which result on receiving the two rays emitted from one crystal of calc spar on to another placed in successively varied positions with respect to the first. Perhaps few points are however easier to exhibit experimentally — which affords by far the readiest way of familiarising ourselves with the whole phenomenon and its laws. It is only necessary to procure two moderately clear rhombs of calc spar, and attach to the side of one of them a card containing a small hole at the centre. It is then easy to look through the two crystals at the light admitted through the small hole; and keeping the two crystals with their surfaces in contact, the one next the eye can be turned round so that its angles point in different directions with respect to those of the other. For this purpose, by far the most convenient arrangement is to fix the two crystals in small tubes (such as card pill-boxes), which can turn one in the other: and if the crystals, and consequently the hole, be small (for the images not to overlap), it is very convenient to magnify the images by a small lens fixed in the tube next the eye, so that the object to be viewed in focus is the small hole at the farthest surface of the second crystal. The series of changes are these: setting out from a position in which the two rhombs are similarly situated (as if parts of one larger crystal), there are two images well separated. These are represented at B in the figure (the two at A being drawn for comparison when only one rhomb is used). Now, making one rhomb revolve contin-

A B C D E F G H I K

uously, we have *four* images (as at C) unequally bright by pairs: at 45° four *equally* bright (D), the other pair now become faint (E), until at 90° they are reduced to two, (F). The same changes are repeated at G and H; when at 135° the four are equally bright, till, after two become faint at I, we arrive at 180°, where, as at K, the two brighter coalesce into one. The same changes then

Thus, then, the two rays on emerging from the first crystal instead of being changed in their nature, are exactly alike; i

take place in reverse order; four images at 225°, two at 270°, four at 315°, an lastly two at 360°.



To give any idea of the analysis of these phenomena, it is necessary in th first instance to observe accurately the form of the crystal, and obtain a distinc idea of the terms the *axis* and the *principal section* of the crystals, which will b understood at once by the aid of the annexed diagram; where taking the *shor* diagonal of two of the opposite faces of the crystal, as A D, the plane passin through it A D X, is the *principal section*, and the diagonal of that plane A X, th *axis* of the crystal. The double refraction of a ray R is represented by its di vision into two rays, O the ordinary, and E the extraordinary.

Whatever theory we adopt as to the nature of light, the phenomena can onl be explained by supposing a section of each of the rays within the crystal to b of an *elongated* form; it may be represented by a short straight line, as O and in the annexed figure (1.): if the rhomb represent a section of the crystal lookin

Fig. 1.

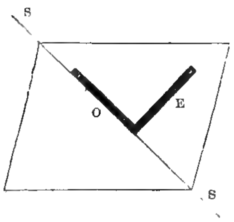
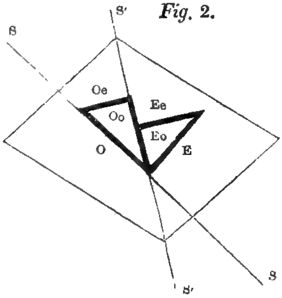


Fig. 2.



down perpendicularly upon it, and supposing the light to fall on it in the sam perpendicular line, S S will be the projection of its principal section, and th short lines O and E will be the projections of the sections of the ordinary an extraordinary rays.

Now let us conceive this first crystal to retain its position, and its princip section S S to remain parallel to itself, as in *fig.* 2., and a second crystal place upon it, having its principal section S′ S′ inclined at any angle to the former then supposing the sections O and E to remain as before, relatively to S S, that one parallel, and the other perpendicular to it, when those rays enter the secon crystal, the effect is that they can only pass through it in such portions as ar either parallel or perpendicular to its principal section S′S′. It becomes then simpl

suffices, to show them undistinguishable from each other, to make one of these rays turn round the line of its own direction through 90°. Thus we are brought by the phenomena of double refraction to distinguish in rays of light *different sides* endowed with different properties. We are brought by observation to acknowledge that the extraordinary ray emerging from a crystal of Iceland spar, has the properties of an ordinary ray if we only make it turn round itself through a quarter of a revolution.

If we call to mind that rays of light are so immensely attenuated that myriads of them can pass through the eye of a needle without mutual disturbance, reflecting minds will recognise how much there is most admirable and almost incomprehensible in the fact which we have just cited, the discovery of which is also due to Huyghens. The two pencils of rays which, after emergence from the crystal of Iceland spar, have sides endued with different properties, are called rays "polarized" in contradistinction to rays of natural light, possessing the same property all round their circumference, since they separate into two beams of the same intensity in whichever direction their *sides* may lie with respect to the form of the crystal with which they are analysed. I have mentioned what ought to be the position of a second crystal, so that the ordinary and extraordinary rays emerging from the first crystal may preserve respectively the same denominations. In the intermediate positions of the second crystal, the rays, whether ordinary or extraordinary, coming from the first, in general divide themselves each into two, but the intensities of the two portions are ordinarily very different.

a case of *resolution of motions*, represented by the lines O, E, and it seems nearly impossible to imagine this without associating it with vibrations. At all events, the only way of conceiving the matter is to admit that in some way O is simply resolved into two components at right angles; one in the plane s′s′, the other perpendicular to it, which are represented by O_o and O_e. In like manner E is resolved into E_o, parallel to s′s′, and E_e perpendicular to it. According to the inclination given to s′s′, relative to s s, the changes in magnitude in these resolved parts will give the relative brightness of the images.

Rays whose sections are represented as in the figure, are said to be polarized in the planes of O and E respectively; but it was long a disputed question whether the *vibrations* of which they consist, according to the wave theory, are actually performed *in* those planes, or perpendicular to them; the latter has now been shown to be the fact.

It need hardly be added that this can be considered only as a very general and popular kind of illustration; and for the more exact statement of the laws of these changes, especially with regard to the relative *distances* of the several images, or differences of ordinary and extraordinary refraction, recourse must be had to more profound mathematical investigations. See especially Herschel on *Light*, art. 785., *et seq.* — *Translator.*

Such was the state of our knowledge on this delicate and singular branch of optics, when, one day, in his house in the Rue d'Enfer, Malus happened to examine, through a doubly refracting crystal, the rays of the sun reflected by the glass panes of the windows of the Luxembourg Palace. Instead of the two bright images which he expected to see, he perceived only one, — the ordinary, or the extraordinary, according to the position which the crystal occupied before his eye. This singular phenomenon struck him much: he tried to explain it by supposing some particular modifications which the solar light might undergo in traversing the atmosphere. But when night came, he caused the light of a taper to fall on the surface of water, at an angle of 36°, and found, by the test of a double refracting crystal, that the light reflected from the water was also polarized, just as if it had emerged from a crystal of calc spar. The same experiment made with a glass reflector at the incidence of about 35°, gave the same result. From that moment it was thus proved that double refraction is not the sole means of polarizing light, or of making it lose the property of dividing itself constantly into two pencils on traversing calc spar. Reflexion at the surface of transparent bodies — a phenomenon occurring every instant, and as ancient as the world — possessed the same property, without being hitherto suspected by any one. Malus, however, did not stop here: he caused an ordinary and an extraordinary ray from calc spar to fall simultaneously on the surface of water, and observed that at the incidence of 36° these two rays acted in a very different manner.

When the ordinary ray underwent a partial reflexion, the extraordinary ray was not reflected at all, — that is, traversed the liquid undiminished. If the position of the crystal was such, relatively to the plane in which the reflexion took place, that the extraordinary ray was partially reflected, then it was the ordinary ray which was entirely transmitted.

The phenomena of reflexion become thus a means of distinguishing from each other rays polarized in opposite directions. On the evening which followed the chance observation of the sun's light, reflected from the windows of the Luxembourg, Malus created one of the most remarkable branches of modern optics, and acquired the title which no one will ever contest to an immortal renown.

I should exceed the limits prescribed me, if I were here to analyse all the observations which our colleague made, in tracing the course of the direct and reflected rays in which the phenomena of polarization were developed. But I cannot omit, in order to

prepare the reader for understanding the curious facts with which Malus enriched the science in 1811, to give the definition of a term which I shall have occasion to employ, that of a ray "partially polarized."

A ray of *natural* light always gives two images of the same intensity, whatever may be the position of the face of the crystal which it traverses, relatively to this ray. A ray *completely* polarized, only gives one image in two particular positions of the face of the crystal. A ray *partially* polarized, possesses in some sort properties intermediate to those of the natural and the completely polarized ray. Like the natural ray, it gives always two images: and as with the polarized ray, these two images have variable intensities, according to the position of the analyser. Rays reflected from water or from glass, at angles greater or less than that of complete polarization, are partially polarized, and in a greater degree, as their inclination to the reflecting surface approaches nearer to 35° or 36° respectively.

Malus conceived that rays reflected from *metals* are not polarized even partially; but this was a slight error which was soon after rectified.

After his first researches, Malus believed that reflexion from certain transparent and opaque substances, besides double refraction, was the *sole* means of polarizing light. About the end of the year 1809, his views on this subject underwent a great extension: he, in fact, recognised, experimentally, that light which has passed through a plate of glass, shows at certain inclinations evident traces of partial polarization; and that if we form a pile of glasses, the natural ray which traverses them emerges completely polarized.

He did not fail to remark, that the polarization of the ray, in this case, was the opposite to that with which the *reflected* ray under the same circumstances was affected: so that if the latter were identified with the ordinary ray, emerging from a crystal placed in a given position, the former, *i. e.* the ray passing through the pile of glass plates, would be similar to the extraordinary ray of the same crystal.

It does not enter into our plan to point out either the detailed and very curious consequences which Malus deduced from his experiments, or the further improvements they have received. I shall content myself by here saying that whenever we find a substance which alone, at the angle of complete polarization, reflects one half of the incident light, the ray transmitted through a single plate will also be completely, instead of partially, polarized. We have no longer need, in order to obtain this complete polar-

ization by refraction, to resort to a pile of glasses as Malus did; a single plate will suffice.*

After the experiments of Huyghens on the double refraction of Iceland spar and of rock crystal†, mineralogists recognised that there exists in nature a great number of crystals endowed with double refraction: but when a crystal was proposed for examination, there was no way of determining whether it could be classed among this description of crystals, until after it had been cut into a prism, and trial made whether the image of a very narrow body, such as the point of a needle, would be double, seen through the two inclined surfaces, whether artificial or natural. But in 1811 a member of the Academy‡ showed that it was possible to decide

* The statement which Arago here gives as to the *complete* polarization of a ray by transmission through a single plate, is a result of the theoretical investigations of Fresnel; being in fact only a particular case of one of his general formulas which include the whole theory of polarization, both complete and partial.

According to Fresnel's principle, *common* light is *equivalent* to a combination of two rays of *equal intensity*, *polarized* in planes at right angles to each other. At reflexion each component gives a reflected and a refracted ray, which, again, are in planes at right angles to each other; but in these rays in the *reflected* pencil it follows, from Fresnel's formulas, that the portion polarized in the plane of incidence will always be of *greater intensity* than the other, and the excess will show itself in the *partially* polarized character of the reflected ray at all incidences; and in the *refracted* ray there will in like manner always be an excess polarized in the plane at right angles to that of incidence. This excess changes with the incidence. At the angle of complete polarization the *whole* of the reflected ray is polarized, but as this amounts to one half the incident ray, the remaining half which is transmitted is also wholly polarized in the rectangular plane.—*Translator.*

† As the discovery of the very small double refractive power of rock crystal has been sometimes ascribed to later experimenters, it may be interesting to give the passage in which Huyghens describes his own observations of it.

He remarks, that his theory seems more probable "from certain phenomena which I have observed in *ordinary crystal which grows in a hexagonal form*, and which, in consequence of this regularity, seems also to be composed of particles of a certain figure, and regularly disposed. This crystal has a double refraction, as well as Iceland spar, though less evident. In cutting it into prisms by different sections, I remarked that in all, looking through them at the flame of a candle, or the leaden divisions of a casement, they appeared double, though with images very little separated; whence I saw the reason why this body, though so very transparent, is useless for telescopes when they are of any great length." *Traité sur la Lumière*, ch. v. § 20.—*Translator.*

‡ Arago here alludes to his own discovery of the polarized tints displayed by any plate of a doubly refracting crystal when interposed between the polarizing and the analysing parts of the apparatus. By this means the eye recognises at once, by the appearance of colour, the existence of double refraction in that crystal plate which might be far too minute in the deviation of images it would give to be detected by the nicest observation; as well as the existence of polarization in any light examined by this test. It was thus that Arago detected polarization in the light of comets, proving that they shine by reflexion.

such questions, without being restricted to the proof, often very difficult, of doubling the image. He proved thus the existence of double refraction in the thinnest plates of mica, which could in no way have been subjected to the former mode of examination. Malus generalised the results thus obtained by his friend, in a memoir entitled *On the Axis of Refraction of Crystals and Organised Substances,* read to the Academy August 19. 1811.

LETTER FROM YOUNG TO MALUS.

On the 22nd of March, 1811, Dr. T. Young wrote to Malus, in terms of great courtesy, to inform him that the Council of the Royal Society of London had awarded to him the Rumford Medal.

So little was the progress which had been made in England in these new theories, that Young requested Malus to assure him whether a ray polarized by reflexion from glass, was really not reflected by a second glass suitably placed, as Malus had announced. In the opinion of the learned Secretary of the Royal Society, the rays which after a first reflexion were incapable of reflexion at a second surface ought to be absorbed or rendered inert.

Again we read in this same letter: "Your experiments demonstrate the *insufficiency* of a theory (that of interferences) which I had adopted, but they do not prove its *falsity.*"

Malus, who was a declared and immoveable partisan of the theory of emission, accepted with great joy the declaration of Young on the insufficiency of the doctrine of interferences. He always held out the opinion of the celebrated Secretary of the Royal Society to those who entreated him to examine, with his superior genius, the hypothesis in favour of which such men as Huyghens and Euler stood so openly committed. He did not remark that Young, in admitting the *insufficiency* of that theory in 1811, had the caution to add that nothing up to that time, even after the discovery of polarization, had proved its *falsity.**

The same principle might be applied, to distinguish *on inspection* a small *fixed star* from an *asteroid*, and thus probably enable astronomers rapidly to discover more of those bodies, were it not that all known forms of polarizing apparatus necessarily involve so great a loss of light, that the method would probably be inapplicable to such faint objects. — *Translator.*

* It may illustrate further the want of due appreciation of the value of Malus's discovery on its first announcement, if, besides the letter of Young here quoted, we refer to several other passages in his correspondence, from which it appears how entirely the discovery of polarization was regarded as something if not quite at variance with the theory of waves, yet as wholly incapable of representation by its principles.

Young, himself, went so far as to predict that it was a problem which "would

INVENTION OF THE REPEATING GONIOMETER.

Physical theories and experimental methods have a mutual reaction on each other. The former cannot be brought to perfection without at the same time inducing a corresponding amelioration in the latter. In proportion as the crystallographic ideas of Haüy acquired more exactness, it was found necessary to employ, for the measurement of the angles of the crystals, methods of increasing precision.

Wollaston supplied this want by the invention of the reflective

probably long remain to mortify the vanity of an ambitious philosophy, completely unresolved by any theory."

Again, in a review of Malus's paper (in 1811), he considers it "conclusive with respect to the *insufficiency* of the undulatory theory in its present state, for explaining all the phenomena of light." And, again, in a letter to Sir David Brewster, five years later, he expresses himself thus:—"With respect to my fundamental hypotheses respecting the nature of light (*i. e.* the wave theory), I become less and less fond of dwelling on them, as I learn more and more facts like those which M. Malus discovers: because, *though they may not be incompatible with those facts, they certainly give no assistance in explaining them.*"[1] Even Malus himself was at first of opinion that the phenomena of polarization were equally irreconcileable with both the undulatory and molecular theories; an opinion which he distinctly expressed in a letter[2] to Young.

Somewhat later, however, we find Young beginning to entertain a more satisfactory view of the case, as appears by the following passage from a letter addressed by him to Arago in 1817:—"I have been reflecting upon the *possibility* of giving an *imperfect* explanation of the affection of light which constitutes polarization, without departing from the genuine doctrine of undulations. It is a principle of this theory that all undulations are simply propagated through homogeneous mediums in concentric spherical surfaces, like the undulations of sound, consisting simply of the direct and retrograde motions of their particles in the direction of the radius, with their concomitant condensations and rarefactions. And yet it is possible to explain in this theory a *transverse vibration*, propagated also in the direction of the radius, and with equal velocity; the motions of the particles bearing a certain constant direction with respect to that radius; and this is polarization."[3]

Now that the idea of transverse vibrations has become familiarised, it seems to present little difficulty; yet it was at first opposed to the prepossessions even of the most zealous undulationists. Fresnel long hesitated fully to adopt the idea, after it had occurred to him as the only mode of representing polarization, on the ground of being unable to reconcile it with mechanical notions; and this more precisely as to the notion of *transverse* vibrations *alone* being produced, which constituted this theory in all its simplicity; whereas Young had (as we have just seen) believed both these and *longitudinal* vibrations to coexist. To establish *this* point, he expressly says, was the main difficulty which embarrassed him.[4]—*Translator.*

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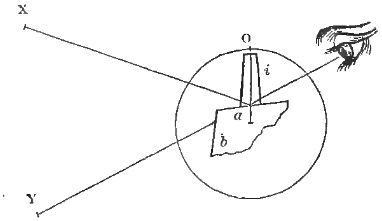
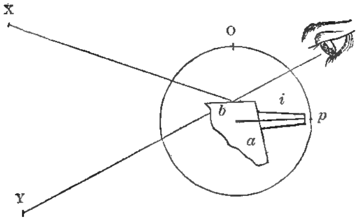
[1] Dean Peacock's Life of Young, p. 379. [2] Works, vol. i. p. 248., note.
[3] Life, p. 390. [4] Ann. de Chimie, 1831, tom. xvii. p. 184.

goniometer which bears his name.* Malus added to the perfection of the English instrument by giving it the principle of *repetition.*†

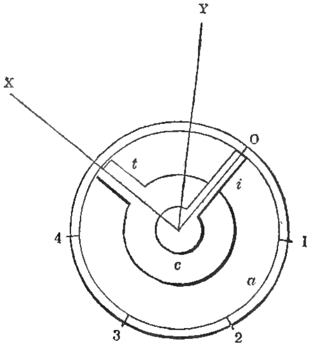
* The essential principle of the reflective goniometer of Wollaston is extremely simple, and consists in this: a piece of crystal or any other object having two plane surfaces *a* and *b*, capable of reflecting light, is fixed at the centre of a graduated circle, to its index *i*. It is first brought into such a





position that the image of an object X, by reflexion from the surface *a*, is seen by the eye coincident with another object Y seen directly; the index marking 0. It is then turned round till the same thing is observed with the surface *b*, when the index marks *p*; the arc O *p* measures the inclination of the two surfaces *a b*, since the surface *b* now occupies the same position with respect to the circle which *a* did before.— *Translator.*

† The principle of "*repetition*" may be thus briefly stated. To *any* graduated



circular instrument intended for measuring the *angular* distance of two objects X Y, there is added an inner circle *c*, moving about the same centre, to which is

He desired thus to be able to compensate the errors of division by successive readings off, and to render the observer independent of the inaccuracies which the artist might have committed in dividing the circles. Unfortunately natural crystals, on which it is possible to use with any advantage the method of repetition, are by no means common. But the method preserves all its theoretical value when it is the object, in optical researches, to measure the angles of prisms formed by truly worked and perfectly polished planes. At the same time it is but just to observe that the idea of employing the reflexion of light for the measurement of angles, is due to the celebrated physicist Lambert.

MALUS A CANDIDATE FOR THE ACADEMY OF SCIENCES. — SITUATIONS WHICH HE FILLED. — HIS DEATH.

The more than ordinary labours of Malus, of which I have just given a rapid analysis, obtained for him the most sincere testimonials of esteem and admiration from men of science of all countries. He was named a member of the Society of Arcueil, which was composed of a small number of men of science assembling under the auspices of Laplace and Berthollet.*

A place in the Section of Physics of the Institute having become vacant in 1810 by the death of Montgolfier, Malus was naturally one of the candidates who presented themselves to fill up the place of the illustrious physicist.

Among the candidates there was conspicuous an engineer of roads and bridges, who had also borne a part in the Egyptian expedition, and whose connections with the academicians were numerous and of old date. Every one, therefore, foresaw that the place would be vigorously contested. On the day of election, August 13.

fixed the part *t*, which (by whatever means) fixes the position of the object; while an index *i* can be either fixed to the inner *c*, or to the outer circle *a*, by clamping, or can move independently. First, the index *i* being clamped to *c*, then pointing to 0, while *t* is directed to the object x; the part *t* is then turned to y, while *i* moves over an arc o 1, equal to that between x and y, and points to 1. Secondly, *i* is clamped to *a* at 1, and unclamped from *c*; *t* is moved back on x; *i* is unclamped from *a*, and clamped to *c*; and *t* moving with *i* is directed to y; *i* consequently comes to 2, passing over an equal arc. Thirdly, the same operation is repeated, and *i* comes to 3, and so on for as many times as may be desired. The arc read off in each instance will, from the errors of graduation, be different. As *any* number of repetitions may be taken, we may have a mean result accurate to any extent desired. — *Translator.*

* The members of the Society of Arcueil were —Laplace, C. L. Berthollet, Biot, Gay-Lussac, Humboldt, Thénard, De Candolle, Collet-Descoutils, A. B. Berthollet, Malus, Arago, Berard, Chaptal, Dulong, Poisson.

1810, one of Malus's friends undertook to bring him the news of the result the moment it was known. But by an unfortunate combination of circumstances the scrutiny was not opened till a later time than usual. Malus obtained 31 votes, his opponent 22. The friend of Malus, just alluded to, did not lose a moment in going to him to announce the happy result. But the usual hour at which the news ought to have reached him having long passed, the great physicist believed himself to have been defeated, and abandoned himself, in spite of all the consolations which his wife afforded him, to the deepest despondency. Thus the intrepid soldier of the army of Sambre and Meuse,—he who had seen the near approach of death at the combat of Chebreys, at the battle of the Pyramids, on the day of the revolt of Cairo, in the immortal day of Heliopolis,—the officer who at Jaffa and Damietta had sustained the attacks of the plague with such firmness of mind,—allowed himself to yield and sink under the supposed want of success in an election of the Academy! Let us preserve and value these recollections! Who will venture to maintain the uselessness of such institutions when he sees the author of one of the greatest discoveries of modern times attach such a price to the title of Academician? Who does not perceive with what emulation young experimentalists ought to be animated, when the society in which they aspire to take their place, constantly anxious to repel from itself all suspicion of party influence, holds itself in the first position in public esteem by taking the greatest care always to recruit its ranks solely from among those who are most worthy.

Malus had become major, a rank corresponding with that of lieutenant-colonel, December 5. 1810. The government had often entrusted him with the mission to classify in their order of merit the officers of artillery and engineers at their departure from the Practical School of Metz. He became afterwards examiner of the pupils of the Ecole Polytechnique for descriptive geometry, and the sciences dependent on it.

On the 14th Vendémiaire an IX.* Malus wrote from Benisouf to his friend Lancret: "I live here like a hermit; I pass whole days without speaking a word." It appeared that our friend often abandoned himself to his taste for silence. The pupils of the Ecole Polytechnique and the Ecole d'Application, related that in going over their exercises he contented himself by pointing out with his finger the parts on which he required explanations, without saying a word. This mode of asking, which contrasted so singularly with that of some other examiners, his contemporaries, not a little

* October 5. 1800.

astonished them. But they did not the less do complete justice to the enlightened patience, the intelligence, and the perfect honesty which characterised all the decisions made by Malus at the close of his examinations. Malus filled, *ad interim*, in 1811, the functions of Director of Studies at the Ecole Polytechnique. There were only wanting some regimental formalities to entrust to him definitively this important employment.

The companion of his choice whom he went to seek at Giessen after the expedition to Egypt, threw over his existence an unspeakable happiness. The most celebrated academies of Europe were envious to secure him as an associate. He was loved, honoured, and esteemed by all who knew him. He might look forward to fresh and brilliant discoveries of which his genius gave promise. He possessed, in a word, after the warlike labours of his youth, all that could attach him to life. It was at this juncture that, to the loss of his connections, of his friends, of the sciences, and the national glory, life failed him.

A consumption, of which he felt the first symptoms about the middle of 1811, made rapid and alarming progress, perhaps from some seeds of the plague which still lurked in his debilitated constitution.

Our colleague did not believe himself fatally attacked; for on the evening before his death, he exacted from one of his friends a promise to accompany him in the course of the week to Montmorency, whither he wished to retire for a short time to breathe the country air. But I can cite a still more demonstrative proof, if possible, of the illusion under which he laboured till the last. Returned from Egypt with the full persuasion that consumption is contagious, and above all that it follows attacks of the plague, he nevertheless allowed Madame Malus, with his head reposing against hers, to watch his least motions, and constantly to be surrounded with the atmosphere which he had breathed.

To the last this admirable woman could not believe in the misfortune which threatened her; and when the illustrious savant breathed his last, it was needful almost to use violence to detach her from the inanimate body of her husband. She survived him only a few months. Malus was only thirty-seven years of age when the Academy lost him.

CHARACTER OF MALUS. — MAXIMS AND PRECEPTS. — SUSCEPTIBILITY OF MALUS ON QUESTIONS OF SCIENTIFIC PRIORITY.

Our colleague was of a middle height and size. In spite of his reserved and cold manners, he had a friendly heart. An excellent son, a tender and irreproachable husband, a devoted friend, — he

has left behind him, in the minds of all who knew him, the reputation, so much to be envied, of a truly good man. His conduct, always beyond reproach even in the most difficult conjunctures, was not merely dictated by an instinctive sense of right. In the leisure of his bivouacs in Egypt he had put down on scattered papers the thoughts and maxims on which he considered that his conduct ought to be modelled. I will here cite some of them which would not disgrace, I think, the most celebrated collections published by any of our philosophers.

"All the actions of life ought to tend towards the perfection of the soul and to social harmony."

"Hope is a source of happiness which is not to be neglected."

"I will found my enjoyments on the affections of the heart, the visions of the imagination, and the spectacle of nature."

"We must exercise patience, as the virtue most absolutely necessary for happiness in our moral existence."

"Mediocrity is a desirable condition of life, since it requires little expense."

"A great part of life often depends on circumstances. There are good things of which we must take advantage as they may occur: — as we enjoy the spring of the year; the brightness of a fine day; or the odour of a rose."

"As we cannot give children the idea of good, we ought to give them the habit of it."

"Even when we stifle reason, conscience comes as a *corps de reserve* to oppose a barrier to irregularity."

"I do not like men who weigh their own good deeds."

I find also in the papers from which the preceding forms a very short extract, a thought expressed in the following terms:—

"One becomes the slave of any man, if injustice on his part can offend and grieve us."

This last precept is full of wisdom; but did the author himself always strictly conform to it? On questions of scientific priority has he not sometimes, to use his own expression, become the slave of his opponents? See and judge for yourselves.

Malus suspected a member of the Institute of Egypt of having wished to invade his rights on the occasion of an analytical calculation being communicated to that learned body. He was so preoccupied with this idea that in a letter addressed to his colleague he omitted to write before his signature, "I am, with consideration, your humble servant." The meaning of this suppression of a customary form of politeness is indicated in positive terms in a letter which I have before me from the officer of engineers to his friend Lancret.

A great geometer conceived the idea of a means of reconciling the phenomena of double refraction with the principle of "least action," and published on this subject a note which everyone may read in our scientific journals.

Malus was convinced that he had himself first conceived the possibility of this investigation, and that he had spoken of it publicly before the publication of that note. He did not content himself with giving publicity to his first ideas without making any mention of the note from the pen of so justly celebrated a writer; but, in spite of his accustomed reserve, he expressed himself on this subject on every occasion with a vehemence of which he would not have been supposed capable.

I will cite a third example: An academician believed he had a right to contest with Malus the priority in an important discovery with respect to polarization. Malus was then at Metz: his letters bear witness, in terms which I know not how to repeat, to his extreme irritation. It appeared to him that the pretensions of his opponent were not well founded in fact, and also that justice enjoined that he should have been allowed reasonable time to explore the first beds of a mine the discovery of which belonged incontestably to him. I ask, nevertheless, whether the susceptibility of Malus can be altogether blamed? Those who defend with so much reason the rights of property as the corner stone of modern civilisation cannot be astonished to see our colleague attach himself with so much ardour to the defence of what is the first and most incontestable kind of property,—that which consists in the works of the intellect. Is it moreover quite certain, when the illustrious physicist showed himself so sensitive on the subject of the fruits of his labours and his genius, that he was not looking forward to one of these solemn meetings where the claims of men of science to the remembrance of mankind are enumerated and appreciated before an enlightened and impartial public, — a judge from whom there is no appeal? Would it then be strange that, seeing himself in imagination before this formidable tribunal, he had dreamed of coming there furnished with the greatest possible number of discoveries uncontested and incontestable? and that under the pressure of these preoccupations he had forgotten for an instant an abstract maxim of philosophy? However this may have been, the integrity and perfect honour of Malus will never be called in question.

In the collection of thoughts from which I have just given extracts, I read:—

"There are very few men, who, when they die, leave behind them any traces of their existence."

I hazard little in asserting that Malus will be reckoned among

these privileged few. His name will go down to the most distant posterity, coupled with one of those great discoveries which, independently of their individual merit, have opened a vast career to the investigations of science. The immortal name of Malus will remain ever inseparable from that of *polarization*, under which all the most curious, the most fertile, the most brilliant phenomena of modern optics are grouped.

FRESNEL.

PRELIMINARY NOTICE.

THE Biography of Fresnel, the first which I had to read, as Perpetual Secretary, at a public meeting of the Academy, gave rise to incidents which several historians of our Revolution of 1830 reported incorrectly. I thus feel myself bound to give the true version of the facts. On arriving at the Academy, July the 26th, 1830, I read in the *Moniteur* the famous ordinances.* I understood in an instant all the political consequences which these acts would bring in their train; I considered them as a national misfortune, and I at once resolved to take no part in the literary solemnity for which we had been convoked. I announced my resolution in these lines, which were to be substituted for the prepared *éloge*:—

"Gentlemen,—If you have read the *Moniteur* your thoughts must doubtless be impressed with a deep sadness, and you will not feel astonished that, for my part, I have not sufficient calmness of mind to be able to take part in this ceremony."

I committed the fault of communicating this resolution to several of my colleagues. From that moment difficulties arose on all sides. "If you execute your project," they said to me, "the Institute will be abolished; now, have you, the youngest member of the Academy, any right to provoke such a catastrophe?" And to support this remark, they pointed out to me several savants whose sole livelihood lay in their appointment as members of the Institute. These observations, strongly represented, shook my determination. The strife nevertheless became hotter; I could consent to read Fresnel's *éloge*, but I obstinately refused to cut out the passages which just before had appeared to be irreproachable, on the necessity to comply with the charter strictly, if it was not wished to open again the career of revolutions. Cuvier, from friendship for me, and also from interest in the Academy, was especially eager to obtain these suppressions. I communicated this circumstance to Villemain, who, without perceiving that the great naturalist was within hearing, exclaimed: "That is signal

* In allusion to the abrogation of the Charter by the ministers of Charles X.

cowardice." From thence quarrels and personalities arose, of which I should feel scruples in depositing the remembrance here. The result, at the time of this lamentable circumstance, was, that the passages in question were preserved in the reading, and became the object, on the part of the public, of enthusiastic applause, which did not appear to be merited either by the matter or the form. I must own that I was much surprised when, on coming out of the meeting, the Duke of Ragusa whispered to me, "God grant that I may not have to go to-morrow to seek for you at Vincennes."

This Biography was read at the Public Meeting of the Academy of Sciences, on the 26th of July, 1830.

Gentlemen,—"There are men who may be succeeded, but whom no one can replace." These words of one of the most honoured writers of our time, so often reproduced as the conventional formula on occasions like the present, are to-day in my mouth the faithful expression of what I feel. How could I, indeed, without the deepest emotion, now occupy before this tribunal a place which has been so worthily filled, during eight years, by the illustrious geometer whose unexpected death has been a source of no less regret to friendship, than to science and to letters.

It is not here, Gentlemen, for the first time that this sincere avowal of my well-founded diffidence has been heard. Nearly all the members of the Academy have in turn been the confidants of my scruples, and their encouraging kindness had scarcely succeeded in surmounting them. Devoted for a long time past to purely scientific researches, entirely destitute of the *literary* claims, which up to this moment had appeared indispensable in the difficult functions which were confided to me, I could only possess in the eyes of the Academy the slight merit of continued zeal, of unlimited devotion to its interests, of an ardent desire manifested on all occasions to see the renown which it had acquired enlarge, if that were possible, and extend itself in all quarters. The void which M. Fourier has left among us (as I was the first to acknowledge, and I acknowledged it without reserve) will be especially felt in these solemn meetings; it is then that you will recal to mind that language in which the most rigorous precision was so happily allied with elegance and with grace. Also I could not but persuade myself that the indulgence of the Academy presaged in some degree that with which the public would deign to honour me; otherwise could I have dared to make an inexperienced

voice heard here, after the eloquent interpreter whom we have just lost, and by the side of him whom we have the happiness still to possess?

I hasten, moreover, to explain that this *éloge* departs from the ordinary form. I shall even ask the favour of its being looked upon as simply a scientific memoir, in which, taking occasion from the labours of our late associate, I have the opportunity of examining the progress which has been made in our times in several of the most important branches of optics. At an epoch when the courses of lectures at the *Collége de France*, of the *Faculté de Paris*, of the *Jardin du Roi* are attracting so great a concourse of auditors, it has occurred to me that the Academy of Sciences might directly address itself to the public (that friend of our studies, showing its goodwill by so numerous an attendance at our meetings) on some of the various questions with which we are specially occupied. At the same time this is but a simple attempt of my own on which I should wish to be enlightened; the critic will find me docile. I hope, however, that the satisfaction of becoming initiated in a few minutes into the most curious discoveries of one century, may appear a sufficient compensation for the inevitable tediousness which so many minute details must cause.

For my own part, the indulgence on which I count will not prevent my making every effort to render myself clear. Fontenelle, on a similar occasion, asked of his auditory (I quote his own expression) "the same attention which they would necessarily give to the romance of the Princess of Clèves if they wished closely to follow the plot, and to know the whole beauty of it." I am aware that I should not be right in demanding so little; but, on the other hand, I have the advantage of speaking before an assembly familiarised with deep study, and from which one may confidently claim a degree of attention which Fontenelle himself, at the commencement of the eighteenth century, would have found it difficult to gain from the frivolous assembly he was addressing.

INFANCY OF FRESNEL. — HIS ENTRANCE INTO THE POLYTECHNIC SCHOOL AND INTO THE CORPS OF BRIDGES AND HIGHWAYS. — HIS DEPOSITION FOR HAVING GONE TO JOIN THE ROYAL ARMY AT PALUD.

Augustine John Fresnel was born the 10th of May, 1788, at Broglie, near Bernay, in that part of the ancient province of Normandy which now forms the department of Eure. His father was an architect, and in this quality had been entrusted by the military engineer with the construction of the Fort of Querqueville, at one of the extremities of the harbour of Cherbourg; but the revolutionary storm having forced him to abandon this work,

he retired with all his family to a moderate property which he owned near Caen, at Matthieu, a little village which already was not without some notoriety, being the birthplace of the poet John Marot, father of the celebrated Clement. Madame Fresnel, whose family name (Mérimée) was also to become one day dear to literature and the arts, was endowed with the most happy qualities of heart and mind; the solid and varied instruction which she had received in her youth enabled her to assist actively, during eight consecutive years, in the efforts which her husband made for the education of their four children. The progress of the eldest son was brilliant and rapid. Augustine, on the contrary, advanced extremely slowly in his studies; at eight years of age he could scarcely read. This want of success might be attributable to the very delicate condition of the young scholar, and to the precautions which it rendered necessary; but it will be still better understood when it is known that Fresnel never had any taste for the study of languages; that he always set very little value on the exercises which address themselves solely to the memory; that his own, which was moreover very rebellious generally, refused almost absolutely to retain words from the moment that they were detached from a clear argument and displaced in arrangement: I must also own, without hesitation, that those whose predictions concerning the future of a child are founded on the precise estimate of the first places which he obtained at the college, in theme or in translation, would never have imagined that Augustine Fresnel would become one of the most distinguished savants of our epoch. As to his young comrades, they had, on the contrary, judged with that sagacity which rarely deceives them: they called him "the genius." This pompous title was unanimously accorded him on account of the experimental researches (I may be allowed this expression, it is but just) to which he devoted himself at the age of nine years, whether for determining the relative length and bore which give the greatest power to the little elder-wood popguns which children use in their play, or in determining which are the woods, dry or green, which are best to use in making bows, under the double consideration of elasticity and strength. The physicist of nine years old had, indeed, executed this little work with so much success, that the toys, hitherto very inoffensive, had become dangerous arms, which he had the honour of seeing proscribed by an express resolution of the assembled parents of all the combatants.

In 1801, Fresnel, aged thirteen, quitted the paternal hearth, and went to Caen with his elder brother. The central school of this town, where the instruction has always been creditable, presented then a *réunion* of professors of the rarest merit. The

excellent lessons in mathematics from M. Quenot, the course of general grammar and logic from the Abbé de la Rivière, eminently contributed to develop in the young pupil that sagacity, that rectitude of mind, which guided him afterwards so happily in the apparently inextricable labyrinth of natural phenomena which he succeeded in clearing. The communication of knowledge is of all the benefits which we receive in our youth that of which a generous heart preserves the deepest remembrance. Hence the gratitude which Fresnel had felt towards his worthy professors at Caen was always lively and respectful. The central schools themselves always occupied a large share of his recollections; and I have some reason to believe that many reminiscences of these ancient institutions would have been found in a plan of study which he wished to publish.

Fresnel entered the Polytechnic School at the age of sixteen and a half, where his eldest brother had preceded him one year before. His health was at that time extremely weak, and gave reason to fear that he would be unable to support the fatigue of so rough a noviciate; but that feeble body enclosed the most vigorous soul, and in all things the firm will to succeed is already half the success; moreover, the dexterity of Fresnel in the graphic arts was nearly unequalled, and on this ground he could fully compete with the cleverest of his comrades, even whilst imposing upon him far less work in a day. When Fresnel went through the course at the Polytechnic School, a savant, whose zeal age has not cooled,— whom the Academy of Sciences has the happiness to number amongst its most active and most assiduous members, and whom, as he is listening to me, I will only designate by the simple title of the chief of living geometers,— fulfilled the duties of examiner. In the course of the year 1804, he proposed to the pupils, as a subject of competition, a geometrical question. Several solved it: but the solution of Fresnel particularly struck the attention of our colleague; for superior men enjoy the happy privilege of discovering, even from slight indications, the talents which will shine brightly. M. Legendre (his name escapes from my lips) complimented the young prize-man publicly. Proofs of encouragement coming from so high a quarter revealed to Fresnel, perhaps for the first time, the secret of his own merit, and conquered an excessive feeling of mistrust, which with him produced the most vexatious results, because it prevented him from attempting new paths.

On leaving the Polytechnic School, Fresnel passed into the department of the "ponts et chaussées." When he had obtained the rank of *ingénieur ordinaire,* he was sent into the department of the Vendée, where the government, desirous to efface the traces of our

deplorable civil discord, raised up all that war had thrown down, opened communications destined to give life to the country, and laid the foundations of a new town. Every *pupil*, whatever may be the career he is about to enter upon, awaits with the most eager impatience the instant at which he may give up that title. To him, in four-and-twenty hours, the appearance of the world becomes completely changed: he has hitherto received instruction; he is going to create it. His future seems, moreover, to promise him all that a century may have offered in the way of brilliant occurrences to some few individuals favoured by fate.

Few engineers, for example, receive their diplomas without believing themselves from this moment called (like new "Riquets") either to join the ocean to the Mediterranean by a great canal which will carry merchantmen even to the centre of a kingdom, or to trace on the slope of the Alps the winding and bold road whose summit is lost amidst eternal snow, but which the traveller nevertheless will face even in the depth of winter. One has conceived the hope of ornamenting the capital with one of those light, and at the same time steady bridges, where the bold chisel of a David may some day come to animate the marble; another, remodelling the gigantic works of Cherbourg, arrests tempests at the entrance of roadsteads, provides useful harbours for merchantmen, associates himself finally with the glory of the national squadrons by furnishing them with new means of attack and defence. The less ambitious have dreamt of improving the course of the principal rivers, and rendering their waters deeper and less rapid by means of embankments; of checking those moving mountains which, under the name of sandhills, gradually invade rich countries and transform them into sterile deserts.

I will not venture to affirm that, notwithstanding the extreme moderation of his desires, Fresnel entirely escaped these happy dreams of youth. At all events the sequel was unexpected. To level small portions of road; to seek, in the countries placed under his superintendence, for beds of flint; to preside over the extraction of the materials; to see to their deposition on the road, or in the wheel ruts; to execute, here and there, a bridge over the irrigation drains; to re-establish some metres of bank which the torrent has carried away in its progress; to exercise principally an active surveillance over the contractors; to verify their accounts; to estimate scrupulously their works,—such were the duties, very useful, though not very lofty, not very scientific, which Fresnel had to fulfil during from eight to nine years in Vendée, in Drôme, and in Ille et Vilaine. How heavily must a mind of such power have been affected, when he compared the use which he might have made of those hours which pass away so quickly, with the way in which they were being spent! But with Fresnel conscien-

tiousness was always the foremost part of his character, and he constantly performed his duties as an engineer with the most rigorous scrupulousness. The mission to defend the revenues of the state, to obtain for them the best employment possible, appeared to his eyes in the light of a question of honour. The functionary, whatever might be his rank, who submitted to him an ambiguous account, became at once the object of his profound contempt. Fresnel could not comprehend the conduct to which persons, in other respects very estimable, believe themselves bound sometimes by an *esprit de corps*. All fraternity ceased for him, notwithstanding the similarity of title and uniform, as soon as any one lost a probity free of suspicion. Under such circumstances the habitual gentleness of his manners disappeared, and gave place to a sternness, I will even say a roughness, which in this age of concessions drew upon him numerous vexations.

The purely speculative opinions of a studious man concerning the political organisation of society, must generally be of too little interest to the public to render their mention necessary; but the influence which they exercised on the career of Fresnel will not allow me to be silent upon them.

Fresnel, like so many good men, associated himself deeply in 1814 with the hopes to which the return of the Bourbon family gave rise. The charter of 1814, executed without retrospective effect, appeared to him to contain all the germs of a wise liberty. He saw in it the aurora of a political regeneration which would, without a check, extend itself from France over all Europe. His patriotic spirit was excited with the idea that our beautiful country was about to exercise such a pacific influence over the good of nations. If, during the Imperial dynasty, the great events of Austerlitz, of Jena, of Friedland, had not strongly excited his imagination, it was solely because they appeared to him destined to perpetuate that despotism under which France at that time bent. The disembarkment at Cannes in 1815 appeared to him an attack on civilisation: and thus, without being hindered by the disordered state of his health, he was anxious to go and join one of the detachments of the royal army of the south. Fresnel flattered himself with the hope of meeting only with men of his own disposition, if I may judge from the painful impression which he experienced at his first interview with the general under whose orders he went to place himself. Touched by the invalid appearance of the new soldier, the general testified his surprise that in such a condition he should expose himself to the fatigues and dangers of a civil war. "Your superiors, Sir," said he, "have enjoined on you this expedition." "No, general," he replied; "I have taken no advice but my own." "I pray you tell me without reserve, has any one

threatened you with not paying your appointments?" "No such threat has been made; my appointments have been regularly paid." "Very well; I ought, between ourselves, to warn you that you can here reckon only on what may be got by chance." "I have reckoned my own resources; I neither hope nor desire any other recompense. I present myself to you to fulfil my duty." "I admire you, Sir; it is thus that every good servant of the royal cause ought to think and act: I participate in your honourable sentiments; you may reckon on my good will."

That good will, in fact, did not fail; and the questions which at first had been painful to Fresnel, showed solely that his questioner, less a novice in the ways of the world, knew by experience that a popular gathering, under whatever colour it may show itself, includes more than a few individuals who under high pretensions conceal personal interests.

Fresnel returned to Nyons, his usual residence, almost dying. The news of the events of the Palud had preceded him. The populace (we know what this term signifies in the south) offered him a thousand insults. A few days afterwards an imperial commissary declared his deprivation of his office, and placed him under the surveillance of the police. Far be it from me to extenuate the odious nature of such a transaction. I ought, however, to say that it was executed without needless rigour, and that Fresnel obtained permission to go to Paris; that he lived there without being disturbed; that he was able to renew his acquaintance with his old fellow students, and to prepare for those scientific researches which he designed to pursue in the retreat where his younger years had been passed. At this time Fresnel had but a very confused idea of the brilliant discoveries which, in the early years of the present century, entirely changed the aspect of optical science.

FRESNEL'S FIRST SCIENTIFIC PAPERS.

The first memoir on science which Fresnel drew up, dates from this same year, 1814. It was an essay whose object was to rectify the explanation, considered as imperfect, of the phenomenon of the aberration of the light of the stars hitherto generally followed in elementary works. Both geometry and physical science equally bore out this new demonstration; but, unfortunately, it too closely resembled that already given by Bradley himself and by Clairault. I say unfortunately, because, if we should suppose that such coincidences are pleasing to the self-love of a *débutant*, or stimulate his zeal, it would be a mistake. On the other hand, an author may support with philosophy, I admit, the unpleasant fact of having uselessly employed his powers for years in the search after a truth

already long since established: he may give up, with the best grace, the flattering hope of seeing his name associated with some brilliant discovery; but might he not feel much more disquieted when there was ground to fear, that from mere ignorance of the existence of prior researches, of which no one dreamed, he might stand charged with plagiarism? when he might apprehend that an irreproachable character was no safeguard against such imputations? The public, notwithstanding the most express denials, will always believe that an author knows all that he might be supposed to know; and the right with which it is invested, to treat with implacable severity those who knowingly borrow from the labours of their predecessors, is the origin of more than one act of injustice. Thus, Lagrange has recounted that in his youth he experienced just such a profound mortification, on finding, by accident, in the works of Leibnitz, an analytical formula which he had completely forgotten, and of which he had spoken to the Academy of Turin as a discovery of his own. From that day he had nearly renounced altogether the study of mathematics. The demonstration of aberration was a matter of too little importance to inspire Fresnel with a similar discouragement; and besides, he had not printed it: but this circumstance rendered him extremely timid; and subsequently he never published any memoir without assuring himself by the testimony of some of his friends, to whom the academical collections were more familiar, that he had not, according to a popular proverb which he habitually adopted, "broken through open doors." *

The first experimental researches of Fresnel do not date earlier than the beginning of 1815: but setting out from this epoch, memoirs succeeded to memoirs, discoveries to discoveries, with a rapidity of which the history of science offers few examples. On

* It is much to be regretted that this early production of Fresnel should not have been preserved — more especially when we recollect that the theoretical explanation of the aberration of light, though apparently well given by Clairault and others, was for a long time by no means clearly apprehended, and far from being exempt from all necessity for further elucidation. In proof of this it may suffice to allude to the fact that, on the occasion of the transit of Venus in 1769, two eminent astronomers, Bliss and Hornsby, calculated the effect of aberration as *accelerating* the phases of the transit, while Professor Winthorp, of Cambridge, U. S., argued that it ought to be that of *retarding* them. Other discrepancies of opinion in past times might also be cited; but the most striking fact has been the controversy in which the whole subject has been involved in our times, arising out of the somewhat startling ideas proposed by Professor Challis, and so largely discussed by that eminent mathematician and Professor Stokes. (See *Philos. Mag.*, 1845-6.) We merely allude to these points in order to show how interesting it would have been to have become acquainted with the view taken of such a subject by a mind so eminently *anticipative* as that of Fresnel.—*Translator.*

the 28th of December, 1814, Fresnel wrote from Nyons: "I do not know what is meant by the polarization of light; beg my uncle, M. A. Mérimée, to send me the best works from which I may obtain information on this subject." Eight months had scarcely elapsed, when highly skilful researches placed him among the most celebrated physicists of our era. In 1819 he carried off the prize proposed by the Academy on the difficult question of diffraction. In 1823 he became a member of that body by an unanimity of suffrages,—a kind of success extremely rare, since it implies not only merit of the highest order, but also, on the part of all the competitors, a frank and explicit avowal of inferiority. In 1825 the Royal Society of London admitted him a foreign associate; and, lastly, two years later, the same body adjudged to him the Rumford Medal. This homage from one of the most illustrious scientific bodies in Europe,—this judgment, pronounced among a rival people, by the countrymen of Newton, in favour of an experimenter who attached little value to his discoveries, except as subverting a system of which that great genius was the defender,—appears to me to possess all the characters of a decree which posterity will confirm. I hope, then, it will be permitted me to appeal to this decree, if in spite of all my desire to confine myself to the strict boundaries of truth, and the consciousness which I have of never having transgressed them, it should happen that this *éloge* should be accused of some exaggeration. Though I must avow it would be a reproach for which I should feel little as the friend of Fresnel, if it were incumbent on me to repel it, it would be solely in the capacity of the organ of the Academy: the office which I this day fill, in the name of my colleagues, ought to be marked by a precision and severity as great as that of the exact sciences with which it is concerned.

REFRACTION.

The labours of Fresnel almost exclusively relate to optics. In order to avoid tedious repetitions, I shall classify them, without regard to the order of dates, in such a way as to collect in a single group all those which relate to analogous subjects. The first which will engage my attention are the phenomena of *refraction.*

A straight rod partly immersed in water appears bent or broken: the rays by which we see the part immersed must, therefore, have changed their route or have been *broken* themselves, in passing out of the water into the air. It was till lately supposed that to this one observation we were to restrict the entire knowledge of the ancients on the subject of refraction. But in exhuming from the dust of libraries, where so many treasures are yet concealed, a manuscript of the optics of Ptolemy, it has been found that the

School of Alexandria had not confined itself to establishing the mere fact of refraction; for this work includes, for all incidences, numerical determinations, tolerably exact, of the deviations of the rays, whether they pass out of air into water or into glass, or whether they enter glass on passing out of water. As to the mathematical law of these deviations, which the Arabian Alhasen, the Pole Vitellio, Kepler, and other physicists had sought in vain, it is to Descartes that we owe its announcement: I say Descartes, and Descartes * alone; for if the later claims put forth by Huyghens

* In thus strongly claiming for Descartes the discovery of the law of refraction which English writers ascribe to Willebrod Snell, Arago might be supposed actuated by a feeling of national pride, which, not unfrequently, perhaps, influenced him on questions of this kind. The strong expression with which he concludes the sentence, seems, however, to point to a more philosophical motive, and to refer the claim of Descartes to considerations derived from the connexion of the law of refraction with his theories. However this may be, it may be well briefly to recapitulate the facts of the case. The ancients, especially Ptolemy, had amassed many measured results. Alhasen (A. D. 1100) stated the general principle that refraction in a denser medium causes the ray to deviate nearer to the perpendicular. Vitellio collected a number of measured results in different media at different angles of incidence; among which Kepler attempted, with his usual ardour, to endeavour to deduce some general numerical relation. He, however, could proceed no further than this — that while the *angle of incidence* is but small, it is in a *constant ratio* (dependent on the nature of the medium) to that of *refraction;* but that, as we deviate more from the perpendicular, the rule becomes less accurate, and soon fails.

Willebrod Snell, in 1621, investigated and established, by comparison of numerical results, a general geometrical mode of representing the case, which, expressed in modern terms, is the true law of refraction (or $\sin i = \mu \sin r$), a constant ratio between the *sines*, not the angles, where i and r are the angles of incidence and refraction, and μ the constant or refractive index. And the relation observed by Kepler, which is true so long as the angle is small enough to be nearly proportional to its sine, is thus extended and generalised. Snell died in 1626 without having printed his discovery; but it had been shown in MS. to many persons, especially to Huyghens, who fully perceived its value and importance. And it is on his authority that the discovery was properly assigned to Snell by Montucla, Bossut, and other writers. Huyghens, however, did not *publish* any account of the matter till it appeared in his *Dioptrica*, which was printed after his death in 1700.

Vossius states that, among others, the contents of Snell's MS. were shown to Descartes.

That philosopher, however, in a manner very usual with him, commences treating the subject on entirely original grounds; and, in the course of a purely theoretical speculation deduces the same law of refraction as a consequence of his *à priori* principles (*Dioptrica*, 1637, ch. ii. § 9.), without making the slightest allusion to Snell. Hence the discovery of the law has been assigned to him, especially by French writers. It is to be observed, however, that he in no way attempts to found his deduction on any comparison of experimental results. Thus, even admitting that Descartes is entitled to the establishment of the law as a *theoretical* deduction, he clearly has no claim to the experimental verification of it, which is by far the most material point; and the more so as his theory is based on the assumption, now proved to be false, that light is *accelerated* in passing through the denser medium.—*Translator.*

in favour of his fellow countryman Snell be accepted, we must give up the pretence of writing the history of science.

A mathematical law has more importance than an ordinary discovery, for it is itself a source of discoveries. From it, simple analytical transformations point out to observers a multitude of results more or less hidden, of which they would with difficulty have become aware; but such results cannot be accepted without reservation, so long as the truth of the primary law rests *solely* on measurements. It is necessary for science that this law should acquire that character of demonstration which mere experiments alone, however precise, cannot confer, by being traceable upwards to the first principles of matter.

Descartes, then, attempted to establish his law of refraction by considerations purely mathematical; perhaps it was thus also that he discovered it? Fermat combated the demonstration of his rival, and replaced it by a method more rigorous, but which had the serious fault of being dependent on a metaphysical principle of which he did not show the necessary truth.* Huyghens arrived

* The theoretical principles here glanced at, are those connected with speculations on one of the most curious points presented by the theory of light; which, perhaps, it may be desirable briefly to explain. Ptolemy had shown that when light is reflected from any surface, the law of reflexion, or equality of angles, is precisely that which causes light to pass from any one point in its course, before incidence to any other in its reflected course, by the *shortest* path and in the *least* time, its velocity being uniform and equal before and after reflexion.

Fermat extended the same principle, called the "principle of least time," to the case of *refraction* according to the law of sines, provided we suppose the velocity *diminished* in the denser medium: that is, he showed that *the sum of the times*, or of the *spaces* DIVIDED *by the velocities*, is a minimum.

Huyghens, adopting the theory of waves, deduced from it the law of the sines; and as, in conformity with that theory, the velocity must be *diminished* in the denser medium, on this theory the principle of "least time" applies to the case of refraction, and that of reflexion also easily follows as a particular case.

On the other hand, on the *molecular theory*, the law of refraction is deduced on the principle of attraction, which the molecules undergo in the medium, and it is a necessary consequence that the velocity must be *increased* in the denser medium. Maupertuis, on these principles, attempted an analogous investigation; but here it was necessary to adopt, not the principle of "least time," but that of "least action," or that the sum of *the* PRODUCTS *of the spaces and velocities* is a minimum; and, on this view, the law of the sines equally results as that which fulfils the condition.

This refers to ordinary refraction: when the same inquiry was extended to double refraction, or to the extraordinary ray, more complex considerations were introduced. This subject is fully discussed by Dr. Young in his Life of Fermat. (*Works*, ed. Peacock, vol. ii. p. 584.) The same principle was the basis of Laplace's investigation of double refraction, of which ("Sur la Loi de la Réfraction Extraordinaire, &c.," *Journal de Physique*, 1809) Dr. Young produced his well-known refutation in the *Quarterly Review* for the same year.

In the case of ordinary refraction, the investigation is very simple. As it is

at the result, setting out from the ideas he had adopted of the nature of light. And, lastly, Newton deduced it from the prin-

not clearly stated, as far as we are aware, in any elementary treatise, it may be satisfactory to some readers to have it briefly put before them.

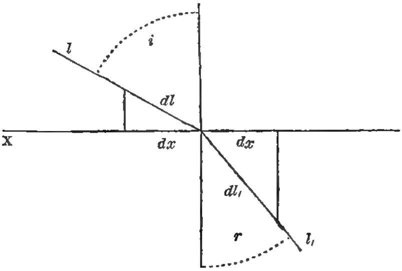
Let any lengths, respectively, of the incident and refracted rays be $l\,l'$, described with the velocities $v\,v'$, which are in a constant ratio to each other; and in times which will be $\frac{l}{v}$ $\frac{l'}{v'}$. Then, on the principle of "least time," the condition is,

$$\frac{l}{v}+\frac{l_{\prime}}{v_{\prime}}=\text{minimum};$$

or, differentiating and multiplying by $v\,v'$,

$$v'\,dl+v\,dl'=0 \quad . \; . \; . \; . \quad (1).$$

Then if x be the surface of the medium, taking equal increments dx on each side of the point of incidence, and dropping perpendiculars to give corresponding



increments $dl\,dl'$, i and r being the angles of incidence and refraction, we have geometrically

$$dl=\frac{\sin i}{dx} \quad dl'=\frac{-\sin r}{dx} \quad . \; . \; . \; . \quad (2);$$

and substituting in (1) it becomes

$$v'\sin i - v\sin r = 0,$$

or $$\sin i = \frac{v}{v'}\sin r.$$

But, as i is necessarily greater than r, it follows that the v must be greater than v': or the law of the sines fulfils the condition of "least time" on the wave theory.

On the other hand, the principle of "least action" requires, instead of equation (1), that we have

$$l\,v+l'\,v'=\text{minimum},$$

or $$v\,dl+v'dl'=0:$$

whence, by precisely the same process, there results

$$\sin i=\frac{v'}{v}\sin r;$$

which can only agree with observation provided v' be greater than v, or the velocity be *increased* in the refracting medium, which agrees with the molecular theory.

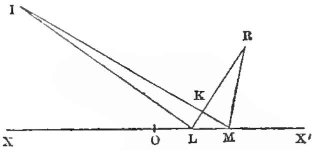
On either supposition, if $v=v'$, and $\sin r$ positive, the case becomes that of *reflexion*, and we have $i=r$, which is the law of reflexion, whence Ptolemy's

ciple of attraction, because that law occupied the attention of the greatest geometers of the seventeenth century.

The question had arrived at this point, when a traveller, returning from Iceland, brought to Copenhagen some beautiful crystals from the Bay of Roërford. Their great thickness and remarkable transparency rendered them particularly proper for experiments on refraction. Bartholinus (1669), to whom they were sent, took care to subject them to different trials: but what was his astonishment when he perceived that the light divided itself into two distinct beams of precisely equal intensities, — when he recognised, in one word, that, seen through the Iceland spar (which has been since found in many other localities, being nothing but carbonate of lime) all objects appear double! The theory of refraction, so many times recast, had now need of a new examination. At all events it was incomplete, for it spoke only of one ray, and two were here seen. Besides, the direction and the magnitude of the deviation of the two rays changed, apparently in the most capri-

conclusion is manifest as a particular case of the general theory. The case of reflexion is, in fact, nothing more than a geometrical problem.

Let two points I R be given without a given straight line X X′, and let O be the point in that line at which straight lines drawn from I and R make equal



angles with X X′. Then taking any other pairs of lines I L, L R, and I M, M R, terminating in the same points and meeting X X′ in L and in M, they will each form unequal angles with X X′; R L X′ greater than I L X, and R M X′ greater than I M X. Let I M and L R intersect in K.

Then we have the angle R L M greater than I L X, which is greater than the opposite and interior I M L; and therefore in the triangle K L M, K M is greater than K L.

In the limit, when M approaches L, we have ultimately I K=I L, and K R= M R; whence I L+L K+K R is less than I K+K M+M R, or the pair of lines nearest to O are together less than the more remote. The same reasoning will apply to all pairs of lines on either side of O; therefore the lines meeting at O are a minimum.

It is an extension of this principle which forms the basis of the investigations of Sir W. R. Hamilton. Observing that in some parallel instances the action is, in fact, not a case of minimum, but of maximum, he has adopted the more generic term, "stationary action;" and upon this has based his fundamental idea of the "characteristic function," by the aid of which his profound analytical system, applicable equally in questions of optics and dynamics, is constructed. For an admirable exposition of the general principle the student should consult Sir W. R. Hamilton's paper on "The Paths of Light and of the Planets" in the *Dublin University Review*, Oct. 1833. — *Translator*.

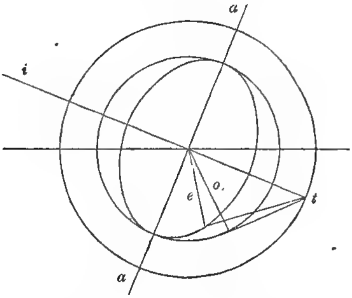
cious manner, when we passed from one face of a crystal to another, or when on one face the direction of the incident ray varied.*

Huyghens surmounted all these difficulties: a general law was found to comprehend in its announcement all the lesser details of the phenomena; but this law, in spite of its simplicity and elegance, was misconstrued. Hypotheses had been for so many ages useless or faithless guides; they had been so long considered as constituting the whole of physics, that, at the epoch of which I speak, experimenters had on this point arrived at a sort of reaction; and in such reactions, even in science, it is rare to be able to keep a just mean. Huyghens had given his law as the result of an hypothesis; men rejected it therefore without examination. The measures on which it was founded could not redeem it from what was thought vicious in its origin. Newton himself took part among its opponents; and from this moment the progress of optics was arrested for more than a century. Since that period, the numerous experiments and measures of two of the most celebrated members of this Academy, Wollaston and Malus, have replaced the law of Huyghens in the rank to which it is entitled.†

* See above, note, p. 384.

† Newton had rejected Huyghens' law, and substituted one founded on measures of his own. In 1788 Haüy repeated the measurements, and showed that Huyghens' rule was far more accurate than Newton's. In 1802 Wollaston repeated similar observations by his new method, in ignorance of Huyghens' law; but found them well represented when that law was pointed out to him—probably by Dr. Young, as the circumstance is stated by him in an article in the *Quarterly Review*, Nov. 1809, p. 338.

Some idea may be given of the simple geometrical construction determining the direction of the extraordinary ray which results from Huyghens' theory, as follows: — Supposing portions of the concentric sphere and spheroid within the crystal, whose axis a coincides with the axis of revolution of the spheroid; and conceiving a second spherical surface concentric, and of greater radius, as



that which would have been the wave surface if the velocity had remained undiminished; then from the extremity t of the incident ray i as if produced to meet this sphere, drawing tangent planes to the sphere and spheroid respectively, the points of contact will give the position of the ordinary and extraor-

During the long discussions which took place among physicists on the mathematical law according to which double refraction is produced in Iceland spar, the existence of the second ray was generally considered as an anomaly affecting half the incident light; the other half, it was said, obeyed the old law of refraction laid down by Descartes: the carbonate of lime, in its crystallised state, then, enjoys certain particular properties, but without losing those which all ordinary transparent media possess. All this was exact in the instance of the Iceland spar, and it seemed as if it might without hazard be asserted generally; but in fact those who maintained this deceived themselves. There are crystals in which the principle of ordinary refraction is not verified; and in which the two rays into which the incident light divides itself *both* undergo anomalous refractions, where the law of Descartes does not indicate the course of either ray.

When Fresnel for the first time published this unexpected result, he had as yet verified it only by the aid of an indirect method, remarkable for the strange circumstance that the refraction of the rays was deduced from experiments in which no refraction took place. Thus our colleague found more than one incredulous reader. The singularity of the discovery, perhaps, demanded some hesitation: perhaps also in the eyes of some persons it had the fault, like the law of Huyghens, of being the fruit of an hypothesis. However it may have been, Fresnel met the difficulty boldly. By showing that in a parallelipiped of topaz, formed of two prisms of the same angle, opposed, no ray passes between the opposite and parallel faces without undergoing deviation, he rendered all objections vain.*

* The paradoxical mention of proofs of refraction, where no refraction takes place, may need a brief explanation.

Fresnel's experiment, here referred to, was performed by means of the simple interference of two rays produced by reflexion from plane mirrors very little inclined from the same plane, or by transmission through a very obtuse-angled prism. If, in the path (as explained in a subsequent note) of each of the two interfering rays, plates of glass of exactly the same thickness are interposed, the position of the stripes remains unaltered; but if the plates be cut from a *biaxial* crystal in different directions with respect to its axis, but still of exactly the same thickness, even if we employ those rays which correspond to the *ordinary* rays in Iceland spar, there will be a displacement of the stripes, showing a difference of velocity or refraction, in these rays (on the principle hereafter explained, see note *infrà*.).

The more direct experiment alluded to consists in this: Fresnel cut two prisms in different directions from the same crystal of topaz, which, being cemented together with their axes in one line, were ground together to exactly the same angle, and the whole achromatised by another opposed prism. On looking through the two prisms thus fixed side by side at a line of light, that line was seen to be broken at the junction, indicating different refractions in the two.

Those physicists (I could here cite the names of some of the most celebrated) who have sought to include in a single rule all the possible cases of double refraction, were thus misled, for they all admitted, as a fact of which no one could doubt, that for half

The law of Huyghens, or the construction of the sphere and spheroid, was found to hold good not only in Iceland spar, but in many other doubly refracting crystals. But these were all characterised by possessing only *one* axis or line along which there was no double refraction, and which, by the aid of polarized light, is easily detected as forming the centre of the rings.

Sir D. Brewster, in examining a vast variety of crystals, discovered a class in which there was not *one* such axis, but *two*, and in which the rings consequently assumed new and more complex forms, being either arranged in two separate sets if the axes were distant, or in coalescing curves if they were close.

For biaxial crystals Huyghens' law will not apply. The incident ray is divided into two; but neither of them follows the law of the sines represented by the sphere in his construction. One of the rays is, indeed, usually less subject to deviation than the other, and thus, for convenience, is still often called the ordinary ray; but both are, in strictness, *extraordinary* rays.

Hence the necessity for a more comprehensive theory. As Huyghens had constructed such a theory by means of an independent sphere and spheroid, Fresnel not only generalised the construction by a method giving two curved surfaces of higher forms, but he did what Huyghens' method did not effect, even in the simple case which he considered,—he showed also a *necessary connexion* between the two surfaces; they were in fact *not two*, but portions of one surface —parts of the geometrical representation of the same algebraic equation, or, in the language of mathematicians, "a curve surface of two sheets." Thus Fresnel's theory showed not only the laws by which each ray was refracted, but also why there must be *two* rays.

Of this more generalised mathematical investigation, the greater part of the steps were omitted by Fresnel in his memoir, as being of too complicated and tedious a nature for the patience of his readers; he presents only the conclusions, which are derived from certain suppositions with respect to the elasticity of the ether, as being different in different directions within the crystal, and ultimately lead to an algebraic equation, representing a curved surface of the fourth order, consisting of two sheets or portions, as the general form assumed by the waves, but which in certain cases, as in calc spar, is reducible to the simpler form of the sphere and spheroid of Huyghens.

For a connected view of these investigations the reader is referred to Professor Powell's *Treatise on the Undulatory Theory, &c.*, p. 48. London, 1841.

The mathematical investigation has since called forth much elucidation, especially in supplying the suppressed processes of Fresnel, in which the analysis of Mr. A. Smith as well as those of Sir J. Lubbock, Professor Sylvester, Sir W. R. Hamilton, and others, have been eminently successful; while the last named mathematician pointed out the very curious consequence that this surface, mathematically speaking, presents, at the extremities of the axis, *conoidal cusps*,—that is, depressions of a pointed funnel shape,—which, physically interpreted, would show that a ray passing along that direction ought to emerge no longer a *single* ray, but spread out in a *conical surface* whose section would not be a *point* of light, but a *ring* with a dark central space. This extraordinary prediction, so wholly unlike anything hitherto imagined, was, however, fully verified by the observations of Dr. Lloyd on a crystal of aragonite; the phenomenon being known by the name of "*conical* refraction."— *Translator.*

the light, for the rays called "ordinary rays," the deviation ought to be the same at the same incidence in whatever direction the plane of incidence cut the crystal. The true law of these complicated phenomena — the law which includes, as particular cases, the laws of Descartes and of Huyghens — is due to Fresnel. This discovery required in an eminently high degree the union of a talent for experiment with the genius of invention.

I freely admit that the phenomena of double refraction recently analysed by Fresnel, and the laws which connect them, are not exempt from a certain complexity. This is indeed a subject of regret — almost, I might say, of lamentation — among some idle minds, who would wish to reduce every science to those superficial notions of which they might make themselves masters by a few hours' work. But does not every one see that with such ideas the sciences would not make any progress; that to neglect such phenomena because one feeble intellect may experience some trouble in grasping them, would be to be false to our vocation, and that thus we should often allow the most important discoveries to pass by us.

Thus astronomy, while limited to a knowledge of the constellations, and to some insignificant remarks on the risings and settings of the stars, was within the capacity of minds of any class: but could we then call it a science? From that time till after the most colossal labour which one man ever went through, — Kepler had substituted elliptic motions not uniform, for the circular and regular motions which, according to the ancients, prevailed in the planets, — his contemporaries might with equal right have complained of complexity. But again, some time after, in the hands of Newton, these motions, complex in appearance, became the basis of the greatest discoveries of modern times, of a principle as simple as it is fertile; they served to prove that every planet is governed in its elliptic course by a simple force, by an attraction emanating from the sun.

Those observers again, who, in their turn refining upon Kepler, showed that simple elliptic motions would not suffice to represent the true paths of the planets, did not *simplify* the science. But besides that the derangement (known under the name of perturbations) would not the less have existed if, in the dislike of all complexity, we had obstinately determined to shut our eyes to them, I ought to say, that in studying them with care we have been conducted, among many other important results, to the means of comparing the *masses* of the different bodies of which our solar system is composed; and that if at the present day we know, for example, that it requires not less than 350,000 times the globe of the earth to form a weight equal to that of the sun, we owe it to

the observation of those very small inequalities, which those would certainly have neglected, who at all risks would admit nothing but *simple* phenomena.

Without extending these remarks farther, I may then admit that optics would be a more easy science, more at the command of the generality of men, more susceptible of demonstration in public lectures, before the extension of it which has been made in our times. But this extension is a real source of riches; it has given occasion for the most curious applications: it has thence afforded those indications of impossibilities in certain theories of light, which may claim to rank among discoveries; for in the search after causes, we are often reduced to proceed by the method of exclusion, and in this point of view, there is no experiment which is without use; we cannot multiply them too much. That universal genius Voltaire, who often took pleasure in concealing a profound meaning under a burlesque form, compared a theory to a mouse, which passes, he said, through nine holes, but is caught in the tenth. It is in multiplying indefinitely the number of these holes, or to speak in a manner less trivial, the number of tests which a theory ought to satisfy, that astronomy is placed in the rank which it occupies in the estimation of men, and that it has become the first of the sciences. It is in following the same route that we shall be able in like manner to give to other branches of science the character of evidence which they yet want in some respects. In every science of observation we must distinguish the facts, the laws which connect them, and the causes. Often the difficulties of the subject arrest experimenters after the first step: hardly ever do they allow them to pass freely to the third. The progress which Fresnel made in the two former respects, in the study of double refraction, by natural consequence, led him to inquire on what so singular a phenomenon depended. And here again he obtained striking success. But pressed for time, I can only make known the most prominent of his results.

When Huyghens published his *Traité de la Lumière*, there were only known two crystals possessing double refraction, carbonate of lime and quartz. At present it would be far shorter to enumerate those which have not this property, than those which have it. Formerly it was necessary that a substance should distinctly show double images, to allow us to assimilate it with Iceland spar. Whenever the separation of the two rays was so small as to escape detection by the eye, the observer remained in doubt and did not venture to pronounce it doubly refractive. Now, however, by the aid of a method which a member of the

Academy has pointed out*, the existence of double refraction manifests itself by characteristics quite independent of the separation of the two images. No substance, however thin it may be, possessed of this property, can escape this new mode of examination. But, if it were certain that double refraction could not exist without our perceiving the very manifest phenomena on which this method is founded, it would not appear equally incontestable that it ought necessarily to accompany them: and a doubt in regard to this, might seem the more natural since the author of this method has himself found certain plates of glass which, without separating the images in a perceptible degree, yet give birth to all the phenomena in question: — since a distinguished philosopher of Berlin, M. Seebeck, afterwards proved that all glass rapidly cooled enjoyed the same property; — and since, lastly, a very able experimenter of Edinburgh † produced the same phenomena by compressing pieces of glass with great force in certain directions. To show that a piece of ordinary glass, thus modified by cooling or compression, always really separates the light into two rays, — and to render this separation incontestably evident, was the important problem which Fresnel proposed to himself, and which he resolved in his usual happy manner.

By placing in the same line, and in a frame of iron carrying powerful screws ingeniously arranged, a number of prisms of glass, which by these screws were subjected to very powerful pressure, Fresnel caused a manifest double refraction to appear. In an optical point of view this assemblage of pieces of common glass became a true Iceland crystal; but here the separation of the images, and all the other properties which flow from it, resulted exclusively from the action of the compressing screws. Now this action, carefully analysed, ought only to produce one effect, a close approach to each other of the molecules of the glass in the direction of the pressure; while, in the direction perpendicular to this, the molecules preserve their original distances. Can we then doubt, after this remarkable experiment, that an analogous arrangement

* The author here alludes to his own discovery of the *polarized colours*, made also quite independently by Brewster about the same time. These tints are now familiar to most persons by means of the little instrument called the polariscope. By placing a plate of selenite, mica, &c., far too thin to exhibit any separation of images, in polarized light, and viewing it through an analyser, these brilliant tints convey distinct evidence of the existence of that property, since they are shown *theoretically* to depend solely upon its *existence*, however insensibly small its *amount* may be. It therefore seems important for the verification of theory, to show independently its existence in any substances which exhibit the tints. Glass ordinarily possesses no such power; but plates of *unannealed* glass exhibit the tints. Hence the importance of the experiments mentioned to show its existence directly.— *Translator.*

of the molecules produced during the act of crystallisation was thus the general cause of the double refraction in carbonate of lime, quartz, and all minerals of the same kind. If we consider with attention the ingenious apparatus, by the aid of which Fresnel, in thus giving an artificial double refraction to ordinary glass, has caused so great a step to be made in the science, we are struck with the great amount of aid which the spirit of invention borrows, whether from the knowledge of the arts, or from that manual dexterity which has been so well described by Franklin when he required of experimenters to be able to saw with a file, and to file with a saw.

Want of time will not permit me to refer here to other various labours of our colleague equally relative to the refraction of light, and of which I do not exaggerate the importance in saying that they would alone suffice to establish a reputation equal to that of many physicists of the first eminence. I hasten to pass on to an optical theory not less interesting, and altogether of modern date; which is designated by the name of the theory of "Interferences." It will furnish me with new occasions to render apparent the astonishing perspicuity of Fresnel's mind, and the inexhaustible resources of his inventive genius.

INTERFERENCES.

The very name of "interference" has as yet hardly emerged beyond the precincts of scientific societies, and yet I know not whether any branch of human knowledge presents phenomena more varied, more curious, more strange. Let us endeavour to disengage the capital fact which pervades this whole theory of the technical language in which it is commonly enveloped, and we may hope it will before long be admitted that it deserves in a high degree to attract public attention.

I will suppose that a ray of the sun's light falls directly on any screen, as for instance on a sheet of fine white paper. The part of the paper on which the ray falls will of course be brightly illuminated; but it might seem incredible if we assert that it depends on the experimenter to render this spot perfectly dark without stopping the ray or touching the paper.

What then is the magical process which allows us to transform at pleasure light into darkness, day into night? The *process* will excite more surprise than even the *result*. It consists in directing upon the paper, but by a route very slightly different, a second ray of light, which, taken by itself, would also have brilliantly illuminated it. The two rays in mixing together, it might be

expected, would produce a yet more brilliant illumination; no doubt, it would seem, could exist on this point: but in point of fact, under certain conditions, they entirely destroy each other, and we find ourselves to have created darkness by adding one portion of light to another.

A new fact requires a new term; this phenomenon, in which two rays in mixing together destroy each other, either wholly or partially, is termed "*an interference.*"

Grimaldi had long ago (before 1665) formed some notion of the action which one beam of light may exercise upon another: but in the experiment which he cites this action was but obscurely manifested; and, besides this, the *conditions* which were essential to its production had not been pointed out, and thus no other experimenter followed up the inquiry.

In searching after the cause of the iridescent colours with which soap bubbles shine so brilliantly, Hooke believed that they were the result of interferences; he even very ingeniously pointed out some of the circumstances which cause their production; but it was a theory destitute of actual proofs. And as Newton, who knew of this theory, did not deign even once in his great work to discuss it critically, it remained more than a century in oblivion.*

The complete experimental demonstration of the fact of interferences will always be the principal title of Dr. Thomas Young to the recognition of posterity. The researches of this illustrious physicist (whose recent loss the sciences have to deplore) had already led to the general principles which I do not think I ought here to abstain from announcing, although the genius of Fresnel seized upon them, extended them, and showed their great fertility.†

* The silence of Newton as to Hooke's attempt at explaining the colours of films by the wave theory may, we conceive, be fully explained from the extremely vague nature of that explanation; it, in fact, amounted to no more than a general notion that some such periodical action, might be occasioned by a concurrence of waves or pulses. It did not amount to a theory: it had no reference to measures of the phenomenon, and indicated nothing like a law. At the time Hooke does not appear to have even been aware of the composition of white light, and thus all accurate analysis of the phenomenon was out of the question. Newton pursued the subject on professedly experimental grounds *alone;* it was not his plan to enter on any theoretical considerations; he, therefore, could not be expected to refer to Hooke's, which must necessarily have seemed to him wholly gratuitous, and even visionary.—*Translator.*

† Young's investigation of diffraction was rather general, and *qualitative*, though the demonstration as to the nature of the effect was perfectly conclusive; but the later researches of Fresnel carried out the subject to a *quantitative* determination. This being made to include the combined effect of an infinite number of interferences acting at every point, involved the use of the higher calculus; and the result was established by means of integrations giving the intensity of light at all parts of the screen or image. This remark applies not merely to the particular case of diffraction, but to that of thin plates, and other

Two rays cannot destroy each other unless they have a common origin; that is to say, unless they both emanate from the same particle of an incandescent body. The rays from one side of the sun's disk do not interfere with those from the other side, or from the centre.

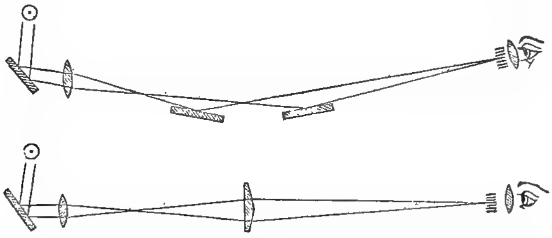
Among the thousands of rays of different tints and refrangibilities of which white light is composed, those only are capable of interference which possess colours and refrangibilities identically the same; thus, in whatever manner we take them, a red ray will never destroy a green ray.

With respect to rays of the same origin and the same colour, they are constantly mixed and superposed without influencing each other; they produce effects represented by the sum of their intensities,—if at the moment of their crossing each other they have gone through routes perfectly equal in length.

An interference can alone take place when the routes through which the rays have passed are unequal: but it is not every inequality of this kind which will necessarily produce a destruction of light: such difference may, on the contrary, cause the rays to reinforce each other.

But when we know what is the *least* difference of route gone through, at which the rays may be superposed *without influencing each other*, we then obtain all the other differences of route which give the same result in a very simple manner; for it suffices to take

analogous cases, in which the principle of interference is applied. This analytical extension constituted one of the most characteristic excellences of Fresnel's researches. In an experimental point of view, Fresnel's researches are characterised by scarcely less improvements. The most material modifications he introduced were those of (1.) viewing the image of the stripes directly by an eye lens, instead of throwing them on a screen;—(2.) discarding any interposition of an opake body, and causing two rays simply to act on each other, by causing the sun's light diverging from a minute aperture or the focus of a small lens, to be divided into two streams, either by reflection from two mirrors very little inclined to each other, or transmission through a very obtuse angled prism.



Here the interference stripes are seen by the eye-glass in the middle of the mixed light in the greatest purity and intensity of alternation of brightness and darkness. — *Translator.*

the double, the triple, the quadruple, &c., i. e. *every* whole multiple, of the first number to give them.

If we have noted in like manner the least difference of route which produces *complete destruction* of the two rays, every *odd* whole multiple of this first number will also be the indication of a like destruction.

As for differences of route which are not numerically comprised either in the first or in the second of the above series, they correspond only to *partial* destructions of the light, or mere weakening of its intensity.

These series of numbers, by aid of which we can tell whether two rays at the moment of intersection ought to interfere or merely to combine without influencing each other, have not the same values for the differently coloured rays; the smallest values belong to the violet rays, the greatest to the red, and the intermediate values to the intermediate rays. It results, that if two white rays cross at a certain point, it may be possible that in the infinite series of differently coloured rays of which that light is composed, the red, for example, alone may be destroyed and disappear, and thus the point of concourse may appear green, as being the white light deprived of its red component.

Interference, then, which in homogeneous light produces only changes in intensity, will manifest itself when we operate with white light in phenomena of coloration. In the course of such singular results we may, perhaps, be curious to find the numerical value of these differences of route, so often mentioned, and which place two rays in the conditions either of accordance or complete destruction. I will mention, then, that for red light we pass from the one of these conditions to the other when we make the difference of route amount to three ten thousandths of a millimètre.*

* The numerical values of the differences of route, as Arago expresses it, or the connection of the wave lengths for different rays with the intervals between the stripes, is easily investigated: and the latter being readily susceptible of accurate micrometrical measurement, the former may be deduced. Let two rays be inclined at a very small angle 2θ. Then the crossings of the waves will give rise to a set of bright and dark points at $+$, o, &c., according as like or unlike portions meet. Let c be the interval between two successive bright points and λ the wave lengths. Then we have obviously the relation —

$$c = \frac{\lambda}{2} \cot. \theta$$

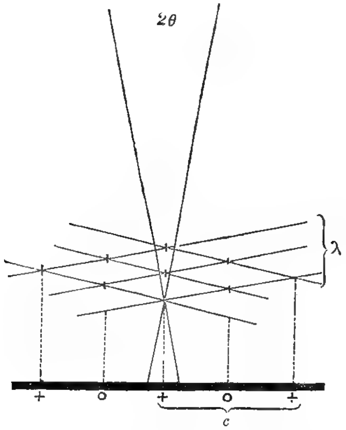
and similarly for successive values of c, measured from the central point, involving successive multiples of λ; and if a plate if glass whose refraction index is μ, be interposed in the path of one of the rays, whose thickness is t, the difference of retardation will be equivalent to a difference of route expressed by

$$d = t\ (\mu - 1)$$

In order that the difference of route alone may determine whether two rays of the same origin and the same colour shall reinforce or destroy each other, it is necessary that both should be traversing *the same medium*, solid, liquid, or gaseous. If it be not thus, we must then also take into account (as a member of the Academy *

and this being substituted for the particular multiple of λ, which expresses the difference of routes in the first formula, gives for the displacement

$$c = t\,(\mu - 1)\,\frac{\cot.\,\theta}{2}$$



It may be added that the values of the wave lengths determined by this method from the observed widths of the stripes, or by others of an analogous kind, give results exactly accordant with those long ago assigned by Newton for the length of the "fits" derived from his measures of the diameters of the coloured rings, and by which, from the known curvatures of the lenses, he determined the thickness of the films, and thence the lengths of the "fits."— *Translator*.

* The retardation of one of the rays, and consequent shifting of the stripes, is here alluded to, which was the discovery of Arago; being in the first instance exhibited by the total disappearance of the stripes, as must be the case if the plate of glass have more than almost an infinitesimal thickness. The fact was first announced as a sort of paradox, that as Young had found the stripes entirely disappear by interposing an opake screen on *one* side only, so Arago produced the same effect with a perfectly transparent screen. In order to explain this effect, let us conceive the simple case of two rays of white light, made to interfere as in Fresnel's experiment.

The slightest consideration will show that, at the middle point of the mixture of light, two concurring rays, of whatever primitive colour or wave length, have gone through precisely the same length of route; and thus the central stripe and its immediate neighbour on each side, are absolutely white and black, and perfectly defined: but in proportion as we recede from this point on either side, the differences of route of the concurring rays become necessarily greater. But white light is a compound of primary coloured rays of different wave-lengths. Hence all the interference stripes, except the exactly central ones, are formed

has proved by incontestable experiments) the thickness and the refractive power of the body through which the rays respectively

by the concurrence of rays having gone through more or less different lengths of route, and consequently with a want of exact concurrence for the different primary rays, which will be greater, as we recede more from the central point; in other words, the stripes towards each side become more and more coloured, and superimposed, till beyond certain limits the stripes disappear, and the whole mixed light is sensibly white.

Now, if owing to any cause one of the two interfering rays were *retarded* in its course behind the other, the two rays would not concur under the same conditions of equal route, as before, at the central point, but it would not be until at some distance *towards the side* on which the retardation took place, that they would be, as it were, placed on equal terms to make up for the retardation in the one by greater length of route in the other; the central point of the stripes, and therefore the whole system with it, would thus be *shifted* towards that side. This may be more clearly illustrated as follows. Let two rays oo' interfere, as in *fig.* 1., arriving simultaneously by an equal number of undulations respectively at u and u', and thus giving rise to a light stripe at the centre of the screen S, which corresponds to the point of concurrence for equal routes, or when the differences for the different colours are insensible. But now, as in *fig.* 2., let the ray o be intercepted by a glass screen G, by which its undulations are re-

Fig. 1.

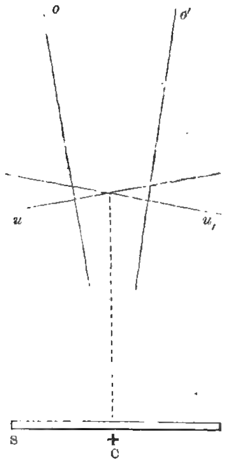
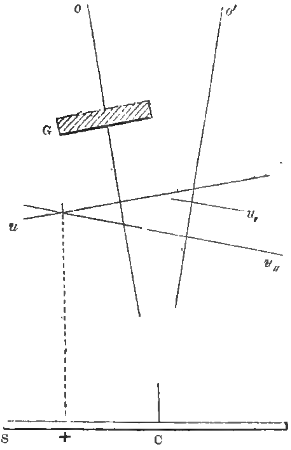


Fig. 2.



tarded. When o' has, as before, arrived at $u_{,}$, o will be at u, several undulations behind it: and the point of concurrence of u with $u_{,}$, will not be the same for different colours, and the central stripe, or point of concurrence for equal *equivalent* routes, will be that with some after wave $u_{,,}$ or will be at + at some distance from C towards G, or the whole body of stripes will be shifted towards the side on which G is placed.

This was accordingly exactly what Arago found to take place when he placed in the path of the light on one side a *transparent* screen. The process by which it is effected is most clearly seen by intercepting the two rays with two plates of glass of exactly the same thickness; and causing one of them to incline very slightly, so

pass. By making the thickness of such media vary gradually, the rays which traverse them may still destroy or reinforce each other, just as if they had traversed routes perfectly equal.

It hardly ever happens that any part of space receives *direct* light alone: a hundred rays from the same origin arrive at that point after reflections or refractions more or less oblique. Now, after what has been said, we may conceive to how many phenomena these repeated crossings of light may give rise; and how superfluous it would have been to seek the reason of them as long as the laws of interference were unknown. Let us only remark that nothing as yet has indicated whether these laws be equally applicable when, before the rays mix, they have received that modification of which I have already spoken, and which is designated by the name of *polarization*.

This question was important: it formed the object of a difficult investigation, which Fresnel undertook in conjunction with one of his friends (Arago). The example which they have set in publishing their researches, of distinguishing which portion each of them contributed, if not with respect to the material execution of the different experiments, at least to the invention of them, deserves, I think, to be followed. For associations of this kind often produce mischief, because the public persisting, often through blind caprice, in not treating the parties concerned on a footing of perfect equality, may improperly excite the self-love of an author; —perhaps of all human passions that which requires the most control.

Let us look at the results of the researches in question, as, without reference to the important *consequences* which have been deduced from them, they deserve to be stated, were it only on account of their intrinsic singularity.

Two rays which are made to change directly from the state of common light to that of rays polarized in the same direction, preserve, after having received that modification, the property of

that the ray on that side passes through a slightly greater *effective* thickness, or is a very little retarded: the stripes are then seen to shift towards that side, until on increasing the inclination, they disappear altogether.

So delicate are the indications afforded by this experiment, and so perfect the accordance between the *degree of shifting* of the fringes, and the *refractive power* of the intercepting medium, that Arago and Fresnel saw the advantage of employing it for the *inverse* problem of determining the most minute differences of *refractive power*, especially those of gases and vapours, for which no other method could be made sufficiently sensible. To demonstrate *at once* the *fact*, and the *law* that this retardation is exactly in proportion to the refractive power of the glass, the translator long ago adopted a simple modification of this experiment, for an account of which the reader is referred to the *Philos. Mag.*, January, 1832.—*Translator.*

interfering as before: they reinforce or destroy each other as ordinary rays do, and under the same conditions. Two rays which change directly from the natural state to that of rays polarzied at right angles to each other, lose altogether the property of interfering: let them be modified afterwards as to the routes they pursue in a thousand ways, or as to the nature and thicknesses of the media they traverse; or even more, let them be brought back by suitable reflections to the condition of parallel polarization; nothing of this kind can give them again the property of being able to destroy each other.

But if two rays already polarized in directions at right angles to each other, and which in consequence cannot act one on the other, have then received *parallel* polarization, in passing out of their natural state, it will suffice, in order that they again acquire the power of interference, to cause them to resume the kind of polarization which they originally possessed.*

* The question as to the nature and modifications of the vibrations whose aggregate in their different stages, or phases, constitutes a wave, may require a word or two of illustration.

In the first instance, in the conception of waves, those who pursued such a theory generally adopted the idea that the æthereal molecules oscillated backwards and forwards *in the line* of the ray: they could not admit the idea of their oscillating in any other direction. Yet, oscillations in any direction occurring in regular succession, might constitute a wave.

The difficulty, when more fully examined, had reference to the determination on admitted dynamical principles of the mode in which the force propagating the ray and acting in its direction could give rise to *lateral disturbance*. Yet it is easy to admit, as a rough illustration, the case of a rope fastened at one end and agitated at the other by the hand; when we can easily cause a series of waves to run along it: but the particles of the rope really retain their original distances from the hand, and merely move up and down in directions *transverse* to its length. In a somewhat similar way, the æthereal molecules are, according to this theory, made to vibrate, or as Fresnel afterwards graphically expressed it, to "tremble laterally."

At length, Young began to entertain the idea that the molecules might oscillate in parallel directions *transverse* to the direction of the ray; though he thought that the longitudinal vibrations might exist also. But he long hesitated to adopt such an idea, regarding it as inexplicable on any dynamical principles. Fresnel independently started the same idea of transverse vibrations, alone; but he was equally reluctant to propose it, on the ground of a similar mechanical difficulty: yet he distinctly acknowledged Young's priority in the announcement of the general idea. "M. Young," he says, "more bold in his conjectures and less confiding in the views of geometers, published it before me, though perhaps he thought of it after me." And on the same point, Dr. Whewell mentions from personal information, that, "Arago was wont to relate that when he and Fresnel had obtained their joint experimental results of the non-interference of oppositely polarized pencils, and that when Fresnel had pointed out that transverse vibrations were the only possible translation of this fact into the undulatory theory, he himself protested that he had not the courage to publish such a conception; and, accordingly, the second part of the memoir was pub-

It is impossible not to feel astonishment, when we for the first time learn that two rays of light can mutually destroy each other,

lished in Fresnel's name alone. What renders this more remarkable is, that it occurred when Arago had in his possession the very letter of Young (1818), in which he proposed the same suggestion." —*Hist. of Inductive Sciences*, ii. 418.

Fresnel deduced transverse vibrations on dynamical grounds which had been open to some degree of question. But the nature of the relation between the partial differential equation which he gives, and the wave function which is the solution of it, clearly involves no necessary restriction of the direction of vibration. That equation is of the same general form as that given by Euler, as referring to sound. Such an equation suffices for light considered as homogeneous. It expresses generally the relation of particles in motion such, that if the time and the position of the particles be increased by corresponding changes, the form of the function will be unaltered, or the motions recur periodically, which constitutes the essential idea of a wave. Its form is generally

$$\frac{d^2u}{dt^2} = -\,\mathrm{c}\,\frac{d^2u}{dx^2}$$

where t is the time, x, the distance along a given axis, and u, the displacement corresponding to the time, t; c, a constant. The solution of this equation is easily seen to be the wave-function.

$$u = \sin\,(nt - kx)$$

Since, if we take the partial differentials in respect to t and to x,

$$\frac{du}{dt} = n\cos\,(nt - kx) \qquad \frac{du}{dx} = k\cos\,(nt - kx)$$

$$\frac{d^2u}{dt^2} = -\,n^2u \qquad \frac{d^2n}{dx^2} = -\,k^2u$$

Whence, $$\frac{d^2u}{dt^2} = -\,\frac{n^2}{k^2}\,\frac{d^2u}{dx^2}$$

And since that wave function goes through all its changes while t, increases to $\frac{2\pi}{n}$ and the velocity $v = \frac{n}{k}$ the time of the undulation $\tau = \frac{2\pi}{n}$ and $v = \frac{\lambda}{\tau} = \frac{\lambda n}{2\pi}$

Whence, $$n = \frac{2\pi v}{\lambda} \text{ and } k = \frac{2\pi}{\lambda}$$

Or the formula becomes (adopting an arbitrary coefficient, a, for the amplitude of vibration which is wholly independent of the other quantities)

$$u = a\sin\frac{2\pi}{\lambda}\,(vt - x).$$

Here it is to be observed, all depends on the coefficient $\frac{n}{k}$ being *constant.* To obtain a similar equation with a *variable* velocity or refraction is the object of the researches of M. Cauchy.

The more extended views of M. Cauchy have led to the deduction of analogous, but more complex, equations, exhibiting resulting expressions for the displacement, in three rectangular directions: besides including in the analysis a coefficient which expresses the variable relation of the velocity which gives the theoretical explanation of unequal refrangibility. These forms thus include the deduction of transverse vibrations, as a direct consequence of the first

that darkness may result from the superposition of two portions of light. But when this property of the rays has been once esta-

assumptions, as to the constitution of an æthereal medium. But, with reference to light, considered as homogeneous, the conditions admit of great simplification; which is best shown in that form of the investigation which was pursued by Sir J. Lubbock (*Philos. Mag.*, Nov. 1837), where, if the fourth powers of the disturbed distances of the molecules are neglected, the equations are at once reduced to the form above.

The object of M. Cauchy's researches here alluded to was to explain the unequal refrangibility of light. To give some general notion of the nature of the subject, we may here briefly observe, that in the explanation of refraction before given [Life of Malus, note,] it is clear that as the inclination of the common tangent to the cotemporaneous circular waves determines the refraction, this depends on the diminution of the wave length within the denser medium; and if this inclination be determined for a ray of any given wave length, then for another whose wave length within the medium is different, and in a given ratio to the former, the radii of the cotemporaneous waves will be in the same ratio as the former, or their difference from the former will be in the same ratio, consequently the common tangent of these second circles will not be parallel to the first, but inclined at a different angle; or the angle of refraction will be different. Thus if for any particular primary ray the wave length within the medium be $\lambda_{,} = \frac{\lambda}{\mu_{,}}$ that of the incident ray being λ and μ the index for that ray, $\lambda = b \sin. i$.

then $$\lambda_{,} = b \sin. r = \frac{\lambda}{\mu_{,}}$$

or in other words, the refraction will be different from each primary ray. But $\mu_{,}$ and $\lambda_{,}$ do not follow any simple ratio. The more complex expression on which that relation depends, is the result of M. Cauchy's theory, viz.

$$\frac{1}{\mu^2} = \text{P} - \frac{1}{\lambda^2}\text{Q} + \frac{1}{\lambda^4}\text{R} - \&\text{c}.$$

[See Professor Powell's Treatise on the Undulatory Theory, sect. vi.]

Experimentally, the transverse vibrations receive their main support from the analysis of the coloured tints, developed in polarized light by the interposition of plates of crystal (such as those of mica, selenite, &c.), when examined by an analyser.

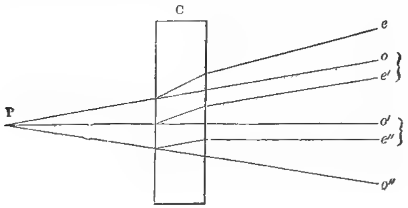
Young ascribed these colours generally to *interference;* but both Fresnel and Arago pointed out that this explanation was incomplete. Why did it only take place in *polarized* light, and even then not until the analyser had been applied? These questions could not be answered till another law had been discovered; as it soon after was, by the joint labours of those two philosophers.

It was clear that in polarization all the vibrations were performed *in one and the same plane*, in whatever direction they might be executed. But it was not until after lengthened investigation that the two philosophers just named succeeded in establishing *experimentally* the important law (obvious as it now seems) that "*polarized rays can only interfere when they are polarized in the same plane.*" If they were polarized in rectangular planes (for example), no interference could result, were all other conditions ever so perfectly fulfilled. Now, this could only be explained on the supposition of the vibrations being performed in planes *transverse* to the ray. Granting that in a ray polarized in *one* plane all the vibrations take place in *one* plane (whether *in* the *same* plane or perpendicular to it), it is then readily seen that when the vibrations of two rays are at *right angles to each other*, there can be no mutual destruction, or mutual

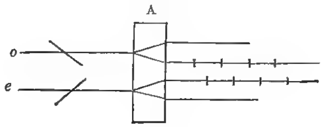
blished, is it not still more extraordinary that we can deprive them of it? that a given ray loses it momentarily, and, that another given ray on the contrary, is deprived of it for ever? The theory of interferences, considered in this point of view, seems more like the reveries of a disordered brain, than the exact, inevitable consequence of numberless experiments, clear of all possible objection. And further, it is not only on account of its singularity that this theory ought to command the attention of the physicist: Fresnel found it the key to all the beautiful phenomena of colours, which are produced in plates of crystal possessing double refraction: he analysed them in all their details: he determined their most hidden laws: he proved that they were only particular cases of interferences. He thus overturned from their base many scientific romances to which these phenomena had given birth,

co-operation. It is only when they are in the *same* plane that this can occur.

This principle was at length found to supply the explanation of the polarized tints. Every ray of the light (P) originally polarized in one plane, in traversing



the crystal plate (C) was divided into two; an ordinary (o) and an extraordinary (e); all those of the one kind o, o', o'', &c. being polarized in one plane, and all of the other e, e', e'', &c., in a plane at right angles to the last. But in each ray o, and e, diverge from each other by a very small angle. The whole pencil also diverges at a small angle from P; thus, the only rays which can *coincide* in direction, will be a ray o, of one set, with a ray e', of the next: — o', with e'', &c. &c., and as these are unequally retarded in different degrees according to their inclination, they would be in a condition to give interference, were it not that being polarized in *places at right angles* to each other they could not. It only required then the action of the analyser (A) to resolve each vibration again



into two, at right angles, of which two sets in a plane perpendicular to that of analysation are suppressed; and two in that plane transmitted: and which, consequently, being in parallel planes, are able to give interference, and produce the observed coloured tints.

and which had secured more than one proselyte, whether by their striking nature or the distinguished merit of their authors. In a word, here, as in every branch of science which is advancing towards perfection, the facts have seemed complicated only because we examined them at too near a distance and with too microscopic a view: but at the same time, by a more enlarged conception, their causes have been found to be more simple than we might have expected.

POLARIZATION.

Although I am aware at what point we risk tiring even the most kindly disposed audience when we speak long on the same subject, I find myself still carried back, by the nature of Fresnel's labours, to the subject of double refraction; but, this time, instead of occupying myself with the manner in which the rays divide in passing through certain crystals, I will examine the permanent modifications which they receive: I will present, in one word, the principal features of the new branch of optics which bears the name of *polarization of light.*

Every ray of light falling even perpendicularly on any surface, natural or artificial, of the transparent crystals of carbonate of lime, called also calc-spar, or Iceland-spar, is divided into two. One portion passes through the crystal without deviation, which we call the ordinary ray; the other undergoes a sensible refraction, and for that reason has very justly the name of the extraordinary ray. Both the ordinary and extraordinary ray lie in one plane perpendicular to the face of the crystal. The consideration of this plane is important, for it is this which determines the direction which the extraordinary ray will take; and in consequence a special name has been given to it, "*the principal section.*"

These points being premised, I will suppose, to fix the ideas, that a particular crystal of calc-spar has its principal section directed north and south. Below this, and at any distance, we will place another similar crystal turned similarly; that is, so that its principal section shall also lie in the meridian. What will result from this disposition, if light traverse the whole system? A single ray impinges on the first crystal but it emerges in two rays: each of these again seems as if it should undergo a double refraction in the second crystal; and thence we might expect four emergent rays. Yet this does not happen. The rays emanating from the first crystal are not divided again by the second. The ordinary ray remains an ordinary ray, and the extraordinary undergoes solely an extraordinary refraction. Thus, in traversing the first crystal, the luminous rays have changed their nature:

they have lost one of their former characteristics, that of constantly undergoing double refraction in traversing Iceland crystal.*

It is necessary that we should fully bear in mind what rays of light are, and then, perhaps, we shall admit that an experiment, by the aid of which they change their original properties in so manifest a way, deserves to be known even by those to whom science is merely an object of curiosity.

The idea which in the first instance presents itself to the mind, when we wish to explain this singular result of which I have just given an account, consists in supposing that in every ray there might exist two distinct species of molecules: that the one species must always undergo the ordinary refraction; the other, the extraordinary alone. But a very simple experiment upsets this hypothesis entirely. In fact, when the principal section of the second crystal, instead of being directed north and south as above supposed, is pointed east and west, the ray which was the ordinary ray in the first crystal, becomes the extraordinary in the second: and reciprocally.

What, then, is there different in reality between the two experiments which give results so dissimilar? There is one circumstance, very simple, and full of import at first sight: it is, that at first the principal section of the second crystal cuts the rays coming from the first through their north and south sides, and in the second case, through their east and west sides.

There must be then, in each of these rays, north and south sides in some way different from their east and west sides. And further, the north and south sides of the ordinary ray ought to have precisely the same properties as the east and west sides of the extraordinary ray: so that if this last ray make a quarter of a circuit about the line of its length it will be impossible to distinguish one from the other. The rays of light are so subtle that thousands of millions of these rays can pass simultaneously through the eye of a needle without interfering: yet we find ourselves obliged to take into account the idea of their *sides*, and to recognise, on their *opposite sides, dissimilar properties.*

When speaking of a *magnet*, natural or artificial, physicists affirm it to have *poles.* They mean only that certain points on its surface are found endowed with certain properties which are not found, or at least only show themselves feebly, at any other points. We have, then, equal reason to say the same thing of the ordinary and extraordinary rays of light which proceed from the division of the beam which passes through Iceland spar; and in contradis-

* For illustration of this subject, see note to the Life of Malus.

tinction to the natural rays, in which all points appear alike, we may rightly call them *polarized rays.*

In order, however, that we may not extend beyond its proper limits the analogy of a polarized ray and a magnet, it is important to remark well that *in the ray*, the *poles diametrically opposite* appear to possess exactly the *same* properties; whilst the *dissimilar* poles are situated on sides of the ray whose positions are *at right angles* to each other.

The lines resembling diameters, which join the *similar* poles, in every ray deserve particular attention. Whenever, in two distinct rays, these lines are parallel, we say that the rays are polarized in the same plane. There is, consequently, no need to add that two rays polarized at right angles to each other must have their similar poles in two directions perpendicular the one to the other.

The two rays, the ordinary and the extraordinary for example, given by any crystal are always polarized at right angles to each other.

All that I have just said of polarization of light was recognised by Huyghens and Newton before the end of the 17th century; and never, certainly, had a more curious subject for research been offered to the meditations of experimenters. Nevertheless, we must pass over an interval of a century after that period before we find, I do not say any fresh discoveries, but even any more researches for the object of carrying out this branch of optics.

The history of all sciences presents a multitude of singular incidents of a similar kind. In the progress of each science there occur periodically certain epochs when, after great efforts, men usually suppose themselves to have arrived at a limit in their advance. Then experimenters are in general timid; they fancy themselves chargeable with a want of modesty, with a sort of profanation, if they dare to lay an indiscreet hand on the barriers which their illustrious predecessors have erected; and thus they generally content themselves with perfecting the numerical elements, or filling up some deficiencies, bestowing on the inquiry a labour often arduous, and which yet scarcely attracts any notice from the world.

In a word, the experiments of Huyghens had clearly established the fact that double refraction modifies the original properties of light in such a manner that, after having once undergone this modification, the rays remain single, or again subdivide into two, according to the direction in which they fall upon a second crystal presented to them. But do these modifications show a relation exclusively to double refraction? do all their other properties remain uninfluenced?

It was from the labours of one of our most distinguished colleagues (like Fresnel, early snatched away from the sciences of which he was the hope) that we have been enabled to answer these important questions. Malus discovered, in fact, that, in the act of reflexion, polarized rays are differently affected from common rays: the latter, as every one knows, are partially reflected when they fall even on transparent bodies, whatever may be the angle of incidence, and whatever the position of the reflecting surface with respect to the *sides* of the ray. When, on the contrary, the case is one of polarized light, there is always one situation of the reflecting surface, relatively to the poles, or sides, in which all reflexion disappears if in this situation the reflexion take place at a particular incidence, which is different for each reflecting surface, according to the nature of the substance of which it is formed.

If, after this curious observation, double refraction ceased to be the *only* means of distinguishing polarized from common light, at least it seemed to be the only way by which rays of light could become polarized. But soon a new experiment of Malus taught the scientific world, to its great surprise, that there existed other methods, far less abstruse, for producing this modification. The most simple phenomenon of optics, the reflexion of light from a transparent mirror, is a powerful means of producing polarization. Light, which is reflected at the surface of water at an angle of 37°, or from the surface of glass at an inclination of 35° 25′, is as completely polarized as the two rays, ordinary and extraordinary, proceeding from a crystal of Iceland spar.

The reflexion of light long ago occupied observers in the age of Plato and of Euclid: since that epoch it had been the object of thousands of experiments, of hundreds of theoretical speculations; the law according to which it proceeds serves as the basis of a great number of instruments, ancient and modern. Among the multitude of enlightened minds, of men of genius, of skilful artists, who, during more than 2300 years, have been occupied with this phenomenon, no one ever aimed at any other object than the means of making the rays divide, or of causing them to diverge or converge; no one ever imagined that reflected light ought not to possess all the same properties as the incident light, or that a change of path would be the cause of a change of nature. Generations of observers thus succeeded each other during several thousands of years, every day touching closely on the most beautiful discoveries without actually making them.

Malus, as I have already explained, gave a means of polarizing light different from that which Huyghens had formerly announced. But the polarizations produced by the two methods were identically

the same. The reflected rays and those which proceed out of an Iceland crystal possess exactly the same properties. Since that time a member of this academy (Arago) has discovered a kind of polarization * entirely distinct, and which manifests itself in a different way from that of difference of intensity. The rays subjected to it, for example, always give two images in traversing calc-spar; but these images are each entirely tinted with a bright and uniform colour. Thus, though the incident light may be white, the ordinary ray may be entirely red, orange, yellow, green, blue, or violet, according to the direction in which the principal section of the crystal cuts the ray: and as to the extraordinary ray, it will not suffice to say that it never resembles the ordinary; we must say that it differs from it as widely as possible; that if the one, for example, is coloured *red*, the other shows a bright *green*, and so on for the rest of the prismatic tints.

When this new kind of polarized rays are reflected from a transparent mirror, we perceive other phenomena not less curious. Let us conceive, in fact, to fix the ideas, that one of these rays be vertical and that it fall on a reflector of pure glass at an angle of about 35°, this mirror may be on the right side of the ray: and the inclination remaining constant, it may be turned to its left, before it, or behind it, or in any intermediate position. We may remember that the incident ray was white; then, in any of these positions of the glass reflector, the ray will not have this colour: it will be now red, now orange, yellow, green, blue, indigo, violet, according to the *side* on which the glass presents itself to the incident ray; it is, in fact, precisely in this order that the tints succeed one another, as we gradually make the mirror go through all possible changes of position. Here there are not only four poles placed in two rectangular directions, which we must admit in the constitution of the ray, but we see that there are thousands; that every point in the circumference round the ray has a special character; that every face which it presents produces in the reflexion a particular tint. This strange *dislocation* of the natural ray (I may be

* It may be necessary for some readers to explain that, in this somewhat paradoxical mode of speaking, the author is referring to his own discovery of the polarized tints: and his meaning is simply that if, in polarized light, there be placed a thin film, *e.g.*, of selenite or mica, and it be viewed through a doubly refracting crystal as an analyser, both the images will be coloured, and their tints complementary. The originally polarized light is divided again into two oppositely polarized pencils in passing through the film, or as Professor J. Forbes has termed it, DI*polarized;* others had termed it DE*polarized.* This is what Arago here calls a new and entirely distinct kind of polarization; though the term is, perhaps, not very happily applied. This is what was explained at large in a previous note.

allowed this word, since it exactly expresses the fact) thus affords the means of *decomposing* white light by means of *reflexion.* The colours, it must be avowed, have not all the homogeneity of those which Newton obtained by the prism; but also the object from which they originate does not undergo any *distortion,* as in prismatic refraction: and in a multitude of researches this is a point of material importance.

To discover whether a ray has received the polarization of Huyghens and Malus, or that of which I have just spoken, and which we call *chromatic polarization,* it suffices, as we have seen, to make it undergo double refraction: but from the fact that a ray in traversing a crystal of Iceland spar always gives two images of white light and of equal intensity, it will not follow that it is formed originally of common light: this is again the discovery of Fresnel.* It is he who first pointed out that a ray may have the same properties round all points of its circumference, and yet not be common light. To show by a single example that these two species of light comport themselves differently, and ought not to be confounded, I will observe that, in undergoing double refraction, a natural ray after traversing a plate of crystal gives two *white* images, while under the same conditions the ray of Fresnel is decomposed into two beams, each *brilliantly coloured.*

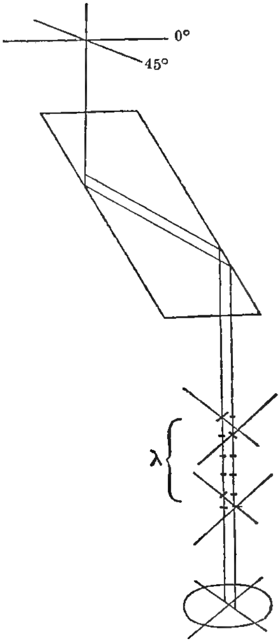
This new modification, which, having no reference to the different *sides* of a ray, has been designated *circular polarization,* can be impressed upon rays ordinarily polarized, by making them undergo two successive *total reflexions* from the internal surfaces of a piece of glass suitably formed.† The pleasure of having his name asso-

* The author would have expressed his meaning more clearly to general apprehension if he had said, that natural or unpolarized white light, on traversing Iceland spar, gives two white images in all positions: an ordinarily polarized ray does not; but there is a kind of light which gives always two images, and yet is not unpolarized: this is the *circularly* polarized light, discovered by Fresnel. One test which distinguishes it from common light is, that *on interposing* a crystallised plate of selenite, mica, &c., before receiving the light on the double refracting crystal, the two images in the former case will be always white, in the latter coloured.

† In the instance mentioned, Fresnel showed, by a remarkable instance of theoretical prediction, that a ray polarized at 45° to the plane of incidence, and *twice* reflected internally from glass, will emerge in the condition of two rays polarized in planes at right angles, and one retarded by one fourth of a wave-length behind the other; these being superimposed will, by mathematical consequence, give rise to vibrations, no longer plane, but performed in circles; or in ellipses, if the retardation be any other fraction of a wave-length. Such a piece of glass is called Fresnel's Rhomb. The course of the ray will be apparent by inspection of the annexed diagram, which needs no further explanation.

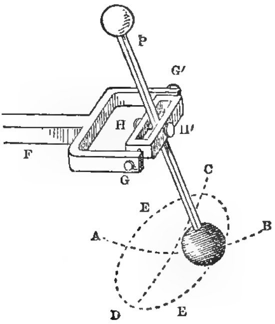
ciated with a new kind of polarization hitherto unsuspected, would probably have sufficed for the vanity of an ordinary experimenter,

The mechanical conception of two rectilinear vibrations at right angles



compounded, giving an elliptical or circular vibration, may be illustrated by a very simple contrivance, which may be described as follows.

On any convenient support, there projects an arm terminating in two branches, on which, by the pivots G G′, a small frame swings. In this frame, by the pivots



H H′, whose axis is at right angles to G G′, a pendulum P vibrates. (The upper end is light, and carries a white ball or disk, carried up to such a height as to be conspicuous for lecture illustrations.)

and his researches would not have extended beyond that point. But Fresnel was actuated by more elevated sentiments: in his eyes nothing seemed to have been done while anything remained to do. He sought, therefore, if there were not other means by which to produce circular polarization; and, as usual, a remarkable discovery was the reward of these efforts. This discovery may be announced in two words; there is a *particular kind of double refraction* which communicates to rays *circular* polarization, as the double refraction of Iceland spar communicates the common polarization of Huyghens. This special double refraction, results not from the nature of the crystal, but from certain sections of it which Fresnel pointed out. The properties of rays circularly polarized also led our colleague to new and very curious means of producing coloured polarization.*

Now, by the pivots H H′ the pendulum can only vibrate in the plane of C D, and by the pivots G G′ it can only vibrate in the plane of A B at right angles to C D. If now motion be given it in one of these planes, and at an *instant after* in the other, the result will be a revolution in the ellipse E E′, which will be a circle if the interval be exactly one fourth of a vibration.

Or mathematically thus :—

Let the waves in planes at right angles, with a difference of retardation d, be expressed by

$$z = a \sin.(nt - kx) \qquad y = \beta \sin.(nt - kx + d)$$

Hence, $\quad \frac{z}{a} = \sin.(nt - kx)$ and $\sqrt{1 - \frac{z^2}{a^2}} = \cos.(nt - kx)$,

or expanding y and substituting

$$y = \beta\left(\frac{z}{a}\cos d + \sqrt{1 - \frac{z^2}{a^2}}\sin. d\right).$$

Whence transposing and squaring

$$\frac{y^2}{\beta^2} + \frac{z^2}{a^2} - \frac{yz}{a\beta}\cos. d = \sin.^2 d$$

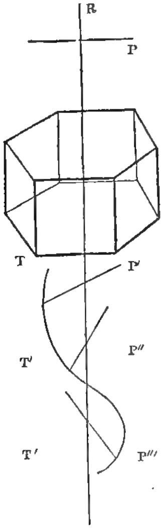
The equation to an *ellipse:* which becomes a circle if $a = \beta$ and $d = 90°$.—*Translator.*

* The author must be supposed here to allude to that remarkable instance of circular polarization which is produced by transmitting a plane polarized ray along the axis of quartz or rock crystal, and which depends, as he says, not on the *nature* of the crystal, but on the *section* of it, that is to say, on the *thickness:* the effect continually *changing* as slices are cut from the crystal perpendicular to its axis of increasing *thickness.* This statement is somewhat remarkable, as he here unequivocally ascribes the discovery to *Fresnel,* which has been usually by English writers ascribed to *himself.*

The term "rotatory" polarization has been since appropriated to describe this phenomenon. Yet the student must be careful to distinguish the application of this term from that of "circular" polarization. The light is in fact circularly polarized: but the effect called "rotation" is quite distinct from the "circularity." It may be desirable to add a brief explanation. Let a ray, R, polarized in a plane P, pass along the axis of rock crystal C, of the thickness T:

In all times and all countries, we find morose dispositions, who, though ready enough to proclaim the glories of the dead, do not treat their cotemporaries with anything like the same favour. As soon as a discovery is announced, they deny its truth; they contest its novelty, and pretend to detect it in some passage of an ancient writer, obscure and forgotten; or, lastly, they maintain that it was only the result of chance.

it emerges polarized in a new plane P′, inclined to P, by a certain angle. If the crystal were of a greater thickness T′, the plane would be turned still further into the position P″, at T″ into P‴, and so on. Thus the successive planes of polarization formed a *twisted surface* like a corkscrew staircase. In some crystals this twisting takes place towards the right in others towards the left. The change of plane is also different for each of the different primary coloured rays. Thus examined by an analyser, the transmitted ray always presents a succession of colours.



Sir J. Herschel showed that the right or left handed character of the polariza tion agreed with the like inclination of the small facets of the complete crysta round the summit. Biot and Seebeck discovered the same property to exist ir certain liquids such as oil of turpentine, and even in some vapours.

The phenomenon is explained theoretically by supposing two rays, eacl circularly polarized in opposite directions, traversing the axis together, but witl unequal velocities. In this case it is shown mechanically that the resultant o such vibrations will be a plane vibration in a continually changing direction proportional to the retardation which one of the rays has undergone, behind th other, in traversing successive thicknesses. This was the discovery of Fresnel For rays deviating a little from the direction of the axis, Mr. Airy showed tha a similar theory would apply with *elliptically* polarized light.

I do not know whether the men of our age are better than their predecessors: but certainly no doubt has been raised either as to the accuracy, or the novelty, or the importance of the discoveries of which I have just given an account. As to the effect of chance, the blindest envy could not dare to appeal to it, so complicated, so minute, and so directly designed for the purpose proposed were the experimental means employed by Fresnel in the study of circular polarization. Perhaps it may be proper to observe that the greater part of them were suggested by theoretical ideas; for without that, most of the experiments of our colleague offer combinations, of which, so to speak, it would seem impossible that any one would have thought. If, in writing the history of the sciences, it is just to put in their full light the discoveries of those who have cultivated them with distinction, it is important also, — it seems to me right, — though freely stating the truth, yet not to put it in such a light as might render it a source of discouragement to any who might be engaged in the same pursuits.

PRINCIPAL CHARACTERISTICS OF THE SYSTEM OF EMISSION AND OF THAT OF WAVES. — GROUNDS ON WHICH FRESNEL WAS LED TO REJECT UNRESERVEDLY THE SYSTEM OF EMISSION.

After having studied with so much care the properties of luminous rays, it was natural to inquire *of what light consists?* This scientific question, one of the grandest, without contradiction, on which men have ever occupied themselves, has given occasion for the most animated discussion. Fresnel took an active part in it. I will therefore endeavour to point out precisely the nature of the question, and give a concise analysis of the experiments to which it has given rise.

The senses of hearing and smell enable us to discover the existence of bodies at a distance by totally different means. Every odorous substance undergoes a species of evaporation: minute particles are sent off from it incessantly, they mix with the air, which becomes a vehicle for them, and diffuses them in every direction. A grain of musk, whose subtile emanations penetrate through all parts of a vast surrounding circuit loses its power from day to day; it ends by being entirely dissipated and totally disappearing.

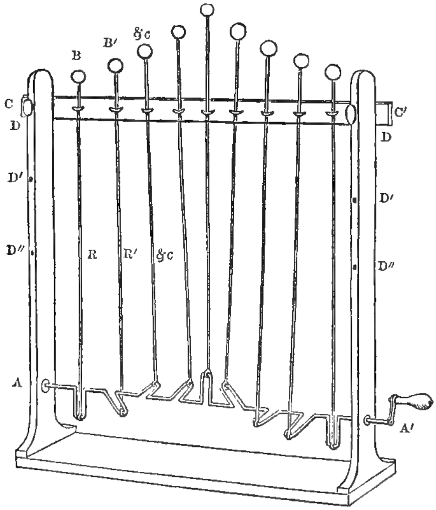
It is not the same with a sounding body. Every one knows that a distant bell, whose sound strikes faintly on our ear, nevertheless does not send to us a single molecule of metal; that it can resound without interruption for successive centuries without

losing any of its weight. When the clapper strikes it, its sides vibrate, they undergo an oscillatory motion which communicates itself immediately to the neighbouring portions of the air, and thence by degrees to the whole atmosphere. These atmospheric vibrations constitute sound.

Our organs, whatever may be their nature, cannot be put in relation with distant bodies, except in one or the other of these two ways: thus either the sun emits incessantly, as odorous bodies do, material particles from all points of his surface with a velocity of 77,000 leagues in a second, and these are minute solar fragments which by penetrating into the eye produce vision;—*or* else that luminary, in this respect like a bell, excites simply an undulatory movement in a medium extremely elastic, filling all space, and these vibrations proceed to agitate our retinas as the sonorous undulations affect the membrane of the tympanum.

Of these two explanations of the phenomena of light, one is called the theory of emission, the other is known under the name of the system of waves.* We find long ago traces of the former

* To assist the general conception of the mode of propagation of waves by transverse vibrations, perhaps it may be desirable to refer the reader to a very simple machine, represented in the annexed figure, contrived by the translator,



which exhibits a set of white balls, representing the molecules of ether these are attached to rods, which are moved on turning the handle by cranks at their lower end, so arranged that each ball is in succession raised or lowered nearly in a straight line; so that they follow each other in the form of a wave. When the bar supporting the rings through which the rods pass, is lowered, the balls no longer move up and down in straight lines, but describe each a kind of

in the writings of Empedocles. Among the moderns I can cite among its adherents, Kepler, Newton, and Laplace. The system of waves does not reckon less illustrious partisans; Aristotle, Descartes, Hooke, Huyghens, Euler, adopted it. Such names on either side render a choice difficult, if in a matter of science the most illustrious names could be authorities capable of determining the point.

If, however, it astonish us to see men of such great genius thus divided, I would say that in their times the question in dispute *could not be resolved;* that the necessary experiments were wanting; and that then the two different theories of light were not logical deductions from facts, but, if I may so express myself, simple matters of persuasion; and that, in a word, the gift of infallibility is not granted even to the most skilful, if they transgress the bounds of observation and, abandoning themselves to conjecture, desert the strict and sure path by which science advances in our age on reasonable principles, and by which it has been enabled to make such incontestable progress. Before we review the great inroads which have been recently made on the theory of emission, it will be perhaps convenient to cast a glance over the vigorous attacks of which it was the object, in the writings of Euler, of Franklin, and others; and to show that the partisans of Newton might then, without looking forward too much, have considered the solution as adjourned for a long period. The effects which a cannon ball can produce depend so directly on its mass and its velocity jointly, that we can, without altering them, change at pleasure one of these elements, provided we make the others change in an inverse ratio. Thus a ball of two kilogrammes may overthrow a wall; a ball of one kilogramme will also overthrow it, provided we impress on it a velocity double of the former. If the weight of the ball were reduced to $\frac{1}{10}$th or $\frac{1}{100}$th of its original amount, to produce the same effect we must give a velocity ten times or one hundred times as great. Now we know that the velocity of a cannon ball is the 640,000th of that of light; if the weight of a luminous molecule were the 640,000th part of that of the cannon ball, it would in like manner overthrow a wall.

These deductions are certain: but let us look at the facts. A luminous molecule not only cannot overthrow a wall, but it even penetrates into an organ so delicate as the eye without occasioning the least pain, without even producing any sensible dynamic effect. We can say more: in experiments undertaken with the view of rendering sensible the impulsions of light, physicists have not been

oval curve, which becomes more rounded the lower the bar is placed. In the former case the machine represents a wave with plane vibrations, in the latter, with elliptic or circular vibrations.

content to use an isolated agent, they have brought to act simultaneously the immense quantity of light which can be condensed at the focus of a large lens; they have not opposed to the shock of the rays very resisting objects, but bodies so delicately suspended that a breath could derange them enormously; they have operated for example, on the extremity of a very light lever suspended horizontally by a spider's thread. The sole obstacle to the rotatory movement of such an apparatus would be the force of reaction, which the thread would acquire in twisting. But this force might be considered as nothing, since from its nature it always increases rapidly with the degree of torsion; and, in this instance, one of the observers whose experiments I am analysing, found no perceptible force of this kind, after having had the patience to give the thread 14,000 turns, by turning the lever round on its centre. It is then well established that, in spite of their excessive velocity, myriads of luminous rays acting simultaneously produce no perceptible force. But we should be going beyond the legitimate consequences which this interesting experiment authorises, if we concluded that a ray is not composed of material elements endowed with a rapid motion of translation. We may, indeed, fairly deduce from the absence of all rotation in the lever suspended by the spider's thread, under the action of an enormous quantity of light, that the elementary particles of the luminous rays have not dimensions comparable to the millionth part of the finest molecules possessing any weight. But as there is nothing to show any absurdity in supposing them a million, or a myriad, times less than this, this kind of experiment and argument (the first idea of which is due to Franklin) cannot furnish any decisive conclusion.

Among the objections which Euler has presented in his works against the theory of emission, I will point out two, on which he has particularly insisted, and which seem to him irresistible. "If the sun," (said this great geometer,) "continually darts out particles of his own substance in every direction, and with enormous velocity, he must end by exhausting himself: and during the many ages which elapsed since the historical period, some diminution ought already to have become sensible."

But is it not evident that this diminution depends on the magnitude of the particulars? Now there is nothing to hinder our supposing them of such small diameters that, after millions of years' continual emission, the mass of the sun should not be sensibly altered. And, besides, there is no accurate observation to prove that this luminary does not waste, or that its diameter is really as great as it was even in the time of Hipparchus.

No one is ignorant of the fact, that millions of rays can penetrate together into a dark room through a pin-hole, and there form

distinct images of external objects. In crossing each other in that minute space, the material elements of which we suppose this multitude of rays to consist ought, nevertheless, to encounter and clash against each other with great violence, to change each other's directions in a thousand ways, and to mingle together without any order. This difficulty is no doubt specious, but it does not appear insurmountable.

The chance that two molecules setting out from the same hole should encounter each other, depends both on the absolute diameter of the molecules, and on the intervals which separate them. We might then by suitably diminishing the diameters reduce the chances of encounter to nothing. But we have here also in the intervals of the molecules another element, which alone would in a great degree lead to the same conclusion. In fact every sensation of light lasts for a certain time: the luminous object which has darted its rays into the eye still remains visible (as experiment has proved) at least for an hundreth of a second after the object has disappeared. Now, in an hundreth of a second, light has gone through 770 leagues. Thus the luminous molecules which form each ray may be at 770 leagues interval from one another, and nevertheless produce a continuous sensation of light. With such distances what becomes of the repeated clashings spoken of by Euler, and which in any circumstances ought to put a stop to the regular propagation of the rays? It is almost humiliating to see a geometer of so rare a genius believe himself authorised by such futile objections to call the system of emission a mistake of Newton,—a gross error,—the belief of which, he says, can only be accounted for by recollecting the remark of Cicero, "There is nothing so absurd but that it has been maintained by some philosopher." *

* It has been too common a practice, both with the advocates and the opponents of the wave theory, to rest its defence or its refutation *on single points:* to uphold a solitary experimental fact as decisive one way or the other. A single favourable fact will not prove the theory; and, on the other hand, the only real conclusion in cases where a single fact appears to stand out as an objection is, that (granting the fact incapable of being otherwise interpreted) the theory requires remodelling; and that some undue assumption has crept into it. Such reconstruction has always been the process by which it has been successively found to adapt itself to new phenomena, even when at first sight they appeared most opposed to it. But even were it otherwise, the theory is one which is not to be staked on single facts: it rests its claim (in the first instance) in being that which *connects* by a common principle, and *thus explains the greatest number* of facts. Many of the old theories, as of inflexion, attractions, &c., each explained a certain small number of facts; but the real argument against them was, that they did not explain each other. Every new partial explanation of the wave theory, on the contrary, not only explains a certain class of facts, but connects these with some other class similarly explained. Newton had proposed one idea (that of fits of easy reflexion and transmission) to account for the alter-

However, the system of emission has few partisans; but it is not under the blows dealt by Euler that it has fallen. Insurmountable objections have been found in various phenomena of whose very existence that philosopher was necessarily ignorant. This great advance in the science belongs to the physicists of our own day, and is due in a great measure to the labours of Fresnel. This consideration alone obliges me to point them out in detail, even if the interest of the question did not oblige me to do so.

If light is a wave, the rays of different colours, similar in that respect to the sounds employed in music, are composed of vibrations unequally rapid; and the red, green, blue, and violet rays, are transmitted through the ethereal spaces, as are all the notes of the gamut through the air, with velocities exactly equal.

If light be an emanation, the rays of different colours are formed of molecules necessarily different, either as to their nature, or their mass, and which besides are endowed with different velocities.

An attentive inspection of the borders of the shadows produced by the satellites of Jupiter in their passage across the luminous disk of the planet, and better still, the observations on changeable stars, have proved that all the rays of light move equally fast. Thus a characteristic feature of the system of waves is found verified.

In each of the two systems of light * the original velocity of a

nations in the colours of thin plates; another totally unconnected theory of inflexion, or bending in and out in passing the edge of a body, to explaint he phenomena of diffraction: a third idea of polarity, for double refraction; besides other occasional references to waves, or even a combination of vibrations with molecular emission in some cases; but all *unconnected with, and independent of, each other*, and each confessedly a mere *arbitrary* assumption, not pretending to stand on any other ground than that it explained in a certain way the particular phenomenon in relation to which it was adduced.

On the undulatory view, on the contrary, every subordinate law successively established, and every class of phenomena explained, has become directly *connected* with all the others. Every part is in intimate relation with every other part, and the progressive improvement and enlargement of the theory has regularly kept pace with the advance of experimental discovery; every new modification, as it were, has grown out of the simple principles at first laid down by a natural sequence, without any new hypothesis, or forced and arbitrary changes. It is a theory of which an eminent philosopher, by no means unduly biassed in its favour, and at a time when it had by no means reached its present point of perfection, emphatically said, "It is a series of felicities; and if not true, eminently deserves to be true." And the increasing proof which it continues to receive by its readiness in meeting nearly every new experimental case as it arises, augments in the same proportion our conviction that it will sooner or later be equally successful in the solution of those few phenomena, which still appear to stand out as exceptional instances to its application.—*Translator.*

* When the author affirms that *in each of the two* theories (dans l'un et dans l'autre des deux systèmes), the original velocity of a ray determines its refrac-

ray determines the refraction which it must undergo when it falls obliquely on the surface of a transparent body. If this velocity

tion, there seems to be a certain degree of confusion, which it is difficult to explain. The assertion is clear, and the whole subsequent argument agrees with that assertion, *in regard to the emission theory.* Here, undoubtedly, the original velocity with which a ray enters a new medium, when it is acted upon by the attractions of a number of surrounding particles, will essentially determine the velocity with which it will continue to move under the influence of these attractions, and the path it will take. But *on the wave theory* there appears nothing obviously and antecedently to show what will be the case.

The author proceeds, *as if continuing the last topic*, to quite another point, viz., the experimental fact that light from the most different sources, both terrestrial and celestial, moves with precisely the same velocity *through air or* vacuum. He argues that this is a "mathematical consequence" of the wave theory; *because*, in the parallel case of sound, tones produced by the most different instruments are propagated through air with the same rapidity. It is certainly a close analogy, but hardly a "mathematical consequence." The remark which follows as to the consequence of molecular theory, in rendering light from different sources unequally rapid in its flight from their differences of attractive power, presents, no doubt, a formidable difficulty to that theory, as being in contradiction to the experimental result just mentioned.

But when, in reference to his own beautiful experiment on observing the refractions of light when its velocity is respectively increased and diminished by the whole velocity of the earth, he adds, "such rays ought to be unequally refracted," I can only understand the meaning by referring to the mention made of the *emission* theory in the next line, and supposing *that* theory alone to be intended. *On that theory*, it is true, such rays *ought to be* unequally refracted.

Observation, however, gives a perfect equality of refraction in the two cases, and thus far completely contradicts the idea of molecular attractions. And when he adds, that the only way in which this contradiction could be reconciled with emission, would be by inventing the subsidiary gratuitous hypothesis that the stars emit an infinite number of rays, endowed with all possible velocities, and that only those of a certain velocity can affect our organs with the sense of vision, this would obviously only be to add "cycle on epicycle," "to save appearances," and would afford no real explanation. On the other hand, with respect to the undulatory theory, it does not appear that it would, on any obvious *à priori* grounds, enable us to predict the result of such an experiment one way or the other. There is indeed involved the difficult and complex consideration of the propagation of vibrations through æther, while the earth and transparent media upon it are moving through that æther: a problem which exercised the ingenuity of Fresnel, and which, after a long investigation, he decided by concluding that the effects would be exactly the same as if the earth were at rest. This, however, may be still regarded as one of those points connected with what is the most difficult part of the wave theory, viz., the primary conception of æther and its properties.

But apart from this consideration, and looking only to the abstract problem of light (suppose emitted on the surface of the earth) falling on a refracting body with *different velocities*, there is nothing apparently *in theory* to determine whether the refraction will be affected, or in what way, by this difference.

On the undulatory principle, it is true, *velocity* is intimately connected with *refraction*: *retardation* and *refraction* being co-extensive and almost equivalent terms: but it must be borne in mind that it is *not absolute* but *relative* velocity

increase, the refraction will be less, and, reciprocally, a diminution of velocity will manifest itself by an increasing deviation. Refraction thus becomes a sure means of comparing the velocities of all sorts of rays. In following out this research with experimental means so precise that they would mark differences of one fifty-thousandth part of the whole amount in question, we have been able to ascertain that the light of all the heavenly bodies, — of our fires, of tapers, and lamps, (with double currents of air,) and even more, the feeble rays emitted by glow-worms, all go through 77,000 leagues in a second, as well as the burning light of the sun.

It is easy to conceive in what way this result is a mathematical consequence of the *system of waves*, if we only remark that all the notes of the musical scale are propagated with equal rapidity through the air, whether they originate from the voice of a singer, from the metallic string of a piano, the catgut of a violin, the glass surface of an harmonica, or the metallic sides of the great pipe of an organ. Now, there is no reason why the *luminous notes* (if I may be pardoned the expression) should proceed differently in æther. *On the hypothesis of emission* the explanation is not so simple. If light is composed of material elements, it would find itself subject to universal attraction: it would with difficulty be

which is thus connected with refraction. It is the *relative* retardation in the denser medium, whatever the *absolute* velocity may be, which causes refraction. If in theory it were shown that the ratio would be constant for all velocities, it would give a constant refraction for the medium. But this is the very point in question: and there appears nothing antecedently to show, on any distinct theory of the nature of æther or of waves, that the relative velocities must necessarily be in a constant ratio. There is, however, nothing in any conception of waves *at variance* with the idea; and it must be admitted as in itself a rational and probable supposition, fairly admissible in the first instance to ground any reasoning upon. When therefore the *fact* was established by Arago's experiment, while it completely subverted what was a *necessary* consequence of the *emission* theory, it offered no contradiction to the undulatory; but the proposition it established being one already *probable*, and *consistent* with that theory, was now to be recognised as an essential part of it. Yet the result of Arago's experiment has been represented by some able writers as of a very startling and unexpected nature, and, at first sight, equally perplexing on either hypothesis.

The undulatory view of refraction depends entirely on the assumption that the velocity must be diminished in the denser medium: but as the refraction is proved to be constant for all velocities, this *diminished* velocity must be always *in a constant ratio* to the *original* velocity. This is the condition to which our conception of æther must conform. As to the *fact* of a *retardation*, that has been directly proved by another beautiful experiment devised by Arago, but carried out by the experimental skill of Foucault, on the principle of Wheatstone's revolving mirror, which, if it received one of two rays at the smallest interval of time *after* another, would reflect it in a *different direction*. Thus the existence of any *retardation* in one of the rays would be manifested.

darted out from an incandescent body, because the attraction of that body will tend to carry it back again; hence a gradual diminution of its original velocity must undoubtedly take place: it is only necessary to inquire whether observation can enable us to discover it. It would be a simple question of calculation how, in making some suppositions with respect to the physical constitution of certain fixed stars in respect to their size and density, which do not appear extravagant, we find that they may, by their attractive force, annihilate altogether the velocity of emission of luminous molecules; that after having proceeded to a given distance, these molecules, which had so far separated themselves from the body, must return thither by a retrograde movement. Thus, certain stars might be as luminous as the sun, to the distance of 40,000,000, leagues, for example, and beyond that be altogether dark; that distance being the exact limit beyond which none of their rays could pass. If we change considerably the volumes and densities which give these results; if we assume for stars of the first magnitude such dimensions as no astronomer would refuse to consider as probable, they will no longer present such strange phenomena, they will no longer be dazzling at this distance, and completely dark at a little farther distance; but the velocity of their light will *change* with the distance: and if two such stars are at very different distances from the earth, their rays will arrive at our eyes with *dissimilar velocities.* Is it not then a formidable objection against the theory of emission, that there should be this perfect equality of velocity in all cases, which all observations testify?

There exist very simple means for altering to a notable extent, if not the absolute, yet the relative velocity of a ray; it is to make observations on it when, in the annual course of the earth, its motion is directed *towards* the star from which the ray proceeds, and again when it is diametrically in the opposite direction. In the former case, it is as if the velocity of the ray was *increased* by the whole of that of our globe: in the second, the numerical difference has the same amount, but the velocity is *diminished.* Now no one is ignorant that the velocity of the earth's revolution is quite *comparable* with that of light; being in fact about a ten-thousandth part of it. And again: to observe a star, towards which the earth is moving, and then one from which it is receding, is to operate upon rays whose velocities differ from each other by one five-thousandth part. Such rays ought to be unequally refracted: the theory of emission furnishes the means of expressing in numbers the amount of the inequality; and we may easily see that it will far exceed the small errors of observation. Now precise measures have completely negatived such calculation:

the rays proceeding from all stars, in *whatever region* they are situated, *undergo precisely the same refraction.*

The disagreement between this theory and experience, could not be more manifest, and from that moment the system of emission seemed to be overturned from its very foundations. Nevertheless this definitive sentence has been suspended by the aid of a supposition which I can explain in two words; — it consists in admitting that incandescent bodies emit rays with all sorts of velocities, but that a special and determined velocity is necessary to make them rays of light. If a ten-thousandth part of increase or diminution in their velocity takes away from rays their luminous properties, the observed equality of deviation is the necessary consequence of this supposition, since in the multitude of rays which strike on the eye, whether it is apparently towards or receding from the stars, it will perceive, in either case, those only whose molecules have the same relative velocity: but this hypothesis, it cannot be denied, deprives the system of emission of the simplicity which constitutes its main recommendation. The clashing of molecules on which Euler so much insisted, would then become the inevitable consequence of their inequality of velocity, and would entail on the propagation of the rays disturbances to which observation does not show them to be subject.

Light exercises a striking action on certain bodies: it rapidly changes their colour. Nitrate of silver, as is well known, possesses for example this power in a high degree. It suffices to expose it for a few seconds to the diffuse light of a cloudy sky for it to lose its original whiteness, and to become of a bluish black. In the rays of the sun it changes almost instantaneously. Chemists have believed that they could see in this discoloration a phenomenon analogous to that they produce every day. According to them the light would be a true "reagent," which in being added to the constituent principles of the compound on which it acts, sometimes modifies its original properties; sometimes also the luminous matter only determines by its action the disengagement of one or more elements of the body on which it strikes.

These explanations, although based on specious analogies, do not seem to be admissible, since it has been shown that, in interfering, the luminous rays also lose the chemical properties with which they are endowed. How can we conceive, in fact, that the matter of two rays can combine with a given substance if each ray strike it singly, while on the contrary no such combination can take place when these same rays strike it together, after having pursued (for this condition is necessary) routes differing from one another by quantities comprised within a certain regular series of numbers?

In geometry, in order to demonstrate the inaccuracy of a proposition, we follow it out to all its consequences until there results something which is completely absurd. Ought we not to class in this category a chemical action which is generated, or which disappears, according to the length of route which the reagent has gone through?

Natural phenomena ordinarily present themselves under very complicated forms, and the true merit of the experimenter consists in disengaging them from a multitude of accessory circumstances which hinder us from at once seizing their laws.

If, for example, we had not observed the shadows of bodies except in the open air, if we had never illuminated these bodies by light proceeding from extremely small luminous points, no one would have guessed how many curious subjects of research are offered by a phenomenon so common. But place in the middle of a dark room, and in a beam of homogeneous light, diverging either from a minute hole, or from the focus of a glass lens, any opaque body whatever, and its shadow will show itself marked by a series of contiguous stripes, alternately bright, and completely dark. Substitute white light for the homogeneous beam, and similar stripes vividly coloured will appear to occupy the place of the former.

Grimaldi first perceived these singular affections of light, to which he gave the name of diffraction. Newton afterwards made them the subject of a special investigation: he thought he saw here the manifest proofs of an intense attractive and repulsive action, which bodies exercise on rays passing close to them. This action, supposing it real, could only be explained by admitting the materiality of light. The phenomena of diffraction, then, deserves in an eminent degree to fix the attention of physicists. Many in fact studied it, but by very inexact methods; Fresnel finally gave to this class of observations a perfection unhoped for, in showing that in order to see these diffracted bands, it is not necessary to receive them on a screen, as Newton and all the other experimenters had done hitherto;—that they are formed distinctly in space, where we can follow them with all the resources which result from the employment of the astronomical micrometer, with a high magnifying power.

According to the precise observations of Fresnel, by the aid of these new modes of observation, if we still wish to attribute the effects of diffraction to attractive or repulsive forces acting on material elements, we must admit that these actions are totally independent of the nature or density of the bodies employed, for a spider's thread and a wire of platinum produce bands exactly the same; the masses have no more influence, since the back and the

edge of a razor produce the same effect. We find ourselves inevitably brought to this conclusion, that a body acts on the rays passing near its surface with so much the less energy as the rays come from a greater distance, since, if on placing the luminous point at the distance of a centimetre, the angular deviation is 12, it will not amount quite to 4 in similar circumstances with light coming from 10 times the distance.

These various results, especially the last, are impossible to reconcile with any idea of an attraction. The experiments of Fresnel destroy entirely all the arguments which had been relied on in the phenomena of diffraction to establish the materiality of light.

That important branch of optics which treats of the intensity of reflected light, transmitted and absorbed by bodies, which is designated by the name of photometry, is but in its infancy; it at present consists of nothing more than isolated results, whose exactness may be open to much question. General mathematical laws are wholly wanting. Some attempts made a few years ago have, however, led to a very simple rule which, for every kind of transparent media connects the angles of the first and second surface at which the reflexions are equal.*

* The measures of intensity of light here alluded to are those of M. Poisson; which, however, were in a great degree anticipated by Dr. Young [Chromatics, *Encycl. Brit.*], though Poisson calls his reasoning indirect, an opinion in which Sir J. Herschel says he cannot concur. Poisson takes the case of perpendicular incidence, and adopts the hypothesis of the vibrations being *coincident* with the *direction of the ray;* he thus obtains expressions for the relative intensities of the incident, reflected, and transmitted rays; and thence, again, of the ray reflected at the second surface. These result in terms of the index of refraction Arago applied this principle (as far as any photometrical measurements can be relied on) for the intensity of light reflected from Mercury to determine its refractive index. The formula of Young is derived from the analogy of the motion communicated from a portion of æther in one medium, to that in a different state of density in another, with that of the impact of unequal elastic bodies, and *without any assumption* as to the *direction* of the vibrations; the same principle on which the formulas of Fresnel are deduced in Mr. Airy's Tract, (Art. 128.) See Sir J. Herschel on Light, Art., 592.; and Lloyd's Lectures on the Wave Theory, p. 31.

Mathematically, Young's formula is deduced in this way. If m and m' be the masses of two elastic bodies, m impinging om m' at rest, by the principles of mechanics (the velocity of m being unity) it is well known that after impact m retains a velocity

$$v' = \left(\frac{m - m'}{m + m'}\right) \qquad (1.)$$

and m' receives a velocity

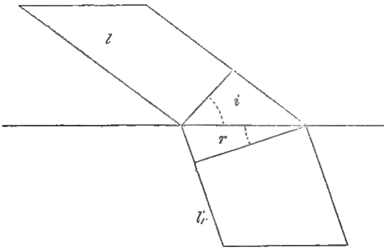
$$v' = \left(\frac{2m}{m + m'}\right) \qquad (2.)$$

It is also *assumed* that this analogy may be applied to a mass of æther (m) in vibration outside the reflecting surface, and communicating its vibrations partly to another mass (m') at rest within the medium: these masses are dependent

In the system of *emission* these two angles have *no necessary dependence;* the *contrary* is the case if the luminous rays are sets

on and partly retaining it in reflexion. Dependent on the densities, in two contiguous media, and the inclination of the ray.

At a *perpendicular* incidence the two masses are simply proportional to the densities or of the refractive powers: or $\frac{m}{m_,}=\frac{1}{\mu}$; hence in this case the velocity of the incident ray being taken as unity, that of the reflected ray will be $\left(\frac{\mu-1}{\mu+1}\right)$ and according to the principle of *vis viva* the *intensity* will be proportional to the *square* of this quantity. This is, however, only a particular case of the general formulas discovered by Fresnel, and applying universally to intensities of reflected light at all incidences. The demonstration of these formulas involves some difficulties which Fresnel did not clear up, but which he, with marvellous sagacity, got over by suppositions somewhat of an empirical and hypothetical kind.[1] To express the masses of the corresponding vibrating portions of æther in the two adjacent media, we take lengths l and $l_,$ of the incident and refracted rays inversely proportional to their refractions or retardations, or inversely as the densities, that is, as sin. r : sin. i; and drawing



parallels to them, the breadths of the parallelograms on the same base are easily seen to be in the ratio of cos. i : cos. r, and thus the ratio of the simultaneously vibrating masses is,

$$\frac{m}{m_,}=\frac{\sin. r \cos. i}{\sin. i \cos. r}$$

Hence Fresnel deduced for vibrations *parallel* to the plane of incidence the ratio of the amplitudes, that of the incident ray being unity,

$$\text{reflected } h' = \frac{\sin. 2\,i - \sin. 2\,r}{\sin. 2\,i + \sin. 2\,r} = \frac{\tan.(i-r)}{\tan.(i+r)} \tag{3.}$$

$$\text{refracted } h_, = \frac{4 \sin. r \cos. i}{\sin. 2\,i + \sin. 2\,r} = \left(1 - \frac{\tan.(i-r)}{\tan.(i+r)}\right)\frac{\cos. i.}{\cos. r.} \tag{4.}$$

For vibrations *perpendicular* to the plane of incidence he found,

$$h' = \frac{-\sin.(i-r)}{\sin.(i+r)} \tag{5.}$$

$$h_, = \frac{2 \sin. r \cos. i}{\sin.(i+r)} \tag{6.}$$

As to the mode of deducing these formulas, considerable discussion has arisen, and the question cannot be regarded as yet settled.

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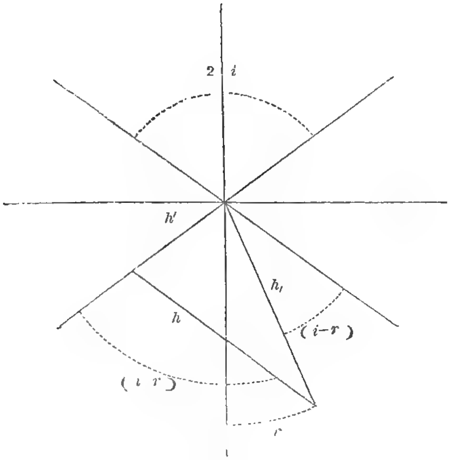
[1] See Mr. Airy's tract of the Undulatory Theory. Art. 128. *et seq.*

of *waves*, and the relation which, in setting out from this hypothesis, one of our most distinguished colleagues has deduced from

On merely geometrical grounds, the *directions* of the incident reflected and refracted rays are seen to form a triangle, whose angles are $(i+r)$, $(i-r)$, and $(\pi-2\,i)$, and their sines being as the opposite sides $h\ h'\ h_{,}$ we have, considering h for the incident ray as unity, values very much resembling the last yet differing from them; viz.: —

$$h' = \frac{\sin.\ (i-r)}{\sin.\ (i+r)} \ (7.) \qquad h_{,} = \frac{\sin.\ 2\,i}{\sin.\ (i+r)} \qquad (8.)$$

If we draw lines perpendicular to the directions of these rays, they will also



form a similar triangle, among whose sides the same relations will subsist. Hence Professor Maccullagh inferred that these would represent the directions of the *vibrations in the plane of incidence*; and as the lengths or amplitudes of these vibrations are unknown, but are dependent upon, as they are the measures of, — the vibratory forces acting, — so if one of the sides of the triangle be assumed to represent the magnitude of the incident vibration, the others will represent those of the reflected and refracted rays, they being the mechanical components of which the former is the resultant.

On this construction, and by assuming the hypothesis of equal density within and without the medium, he deduced the above formulas (7.) (8.) for vibrations *parallel* to the plane of incidence, and others *resembling* (3.) and (4.) for vibrations *perpendicular* to that plane, thus differing essentially from the former. See Professor Maccullagh's paper "On the Laws of Crystalline Refraction," Transactions of the Irish Academy, vol. xviii.; and Dr. Lloyd's Lectures on the Wave Theory, part ii. p. 30. The whole subject has been fully discussed by the Translator in three papers in the Philosophical Magazine for July August, and October, 1856.

The demonstration in either case is grounded on the assumption of the law o *vis viva*; viz.:

$$m\,(h^2 - h^{12}) = m_{,}\,h_{,}^{1}$$

his scientific analysis is precisely that which experience has furnished. Such an accordance between calculation and observation

And Fresnel's formulas would be directly deduced *if* we had also the relations

$h + h' = h_{\prime}$ for vibrations perpendicular to the plane of incidence,

and $h - h' = h_{\prime} \dfrac{\cos. r}{\cos. i}$ . . . . parallel to the plane of incidence.

The difficulty is, that these formulas are not *both* deducible from the principle of equivalent vibrations as laid down by Professor Maccullagh. Another mode of deduction, on a different assumption, is pointed out in the Philosophical Magazine for Oct. 1855, by means of the geometrical construction above given.

The theory of Fresnel, it will be easily seen, is equivalent to the assertion that "the plane of vibration is *perpendicular* to the plane of polarization," whereas in that of Maccullagh they are *coincident.*

Several classes of experiments have been now shown to necessitate the adoption of the former view: for an account of which the reader is referred to the Philosophical Magazine for Aug. 1856, before cited.

To proceed to the *applications* of these formulas: we may consider common light as consisting of two portions of equal intensity, polarized at right angles to each other. If the intensity of the incident light be 1, that of each of these components will be $\frac{1}{2}$. At reflexion each component gives a reflected and a refracted ray polarized respectively at right angles. In the reflected ray the intensity of the portion polarized in the plane of incidence (I) will be $= \frac{1}{2} h'^2$. That in the plane perpendicular to (K) will be $= \frac{1}{2} k'^2$, and it is easily seen, from the nature of the fractions, that of these quantities the first will always be the greater; and thus in their sum or the total intensity there will be an *excess* of light polarized in the *plane of incidence*, or the light is at all incidences partially polarized in the plane of incidence. The difference of the two expressions gives the quantity of light so polarized.

In the refracted ray the intensities of the residuary portions respectively will be

$$\tfrac{1}{2}\,(1-h'^2) \text{ in I}$$
$$\tfrac{1}{2}\,(1+k'^2) \text{ in K}.$$

Here the second is always the greater: and the refracted ray contains an *excess* of light polarized *perpendicularly to the plane of incidence.* The difference or quantity of light polarized is *the same* as in the reflected ray. Hence the light will be completely polarized at any incidence for which either of the expressions (3.) or (5.) vanishes. No value of i will make (5.) vanish, since we can never have $i=r$. But the expression (3.) becomes $=0$ when $i+r=90°$. In this case *the light is completely polarized in the plane of incidence.* But in this case we have also

$$\cos. i = \sin. r = \frac{\sin. i}{\mu} \text{ or } \tan. i = \mu,$$

which is Brewster's law; also if $i + r > 90°$ we have $-\tan. (i + r)$.

Also at this incidence $\frac{1}{2}$ the incident light is reflected, wholly polarized in I $\frac{1}{2}$ is also transmitted wholly polarized in K. *This is the case referred to by Arago in the text.* From (5.) also another remarkable inference follows: if the reflexion be internal, or the ray be incident on the second surface of a dense medium, we have r greater than i, or

$$+\frac{\sin. (i - r)}{\sin. (i + r)};$$

that is, the *phase* of the reflected vibration is changed by 180° equivalent to a

ought at the present day to take its place among the most forcible arguments which we can produce on which to support the system of vibrations.

difference of $\frac{\lambda}{2}$ in route, from what it would be in reflexion at the first surface at the same incidence. *This explains the supposed assumption of the half undulation in Newton's rings.*

Again: if a polarized ray be incident on a reflecting surface with its plane of vibrations inclined to the plane of incidence (I), at an angle (α), its vibration (h) may be resolved into two, one in the plane (I), and one perpendicular to it (K), in the ratio of sin. α and cos. α, or after reflexion we shall have for the respective amplitudes (5.) and (3.)

$$k' \sin. \alpha, \text{ and } h' \cos. \alpha.$$

These by composition will give a resultant ray polarized in a plane (P), inclined to (I) by angle (β), and we have, from the formulas (5.) and (3).

$$\tan. \beta = - \tan. \alpha \frac{\cos. (i + r)}{\cos. (i - r)}$$

This formula exhibits remarkable changes at successive incidences: at incidences *less* than that of complete polarization, the new plane of polarization (as indicated by the sign of the tangent) deviates on the side of the plane *opposite* to that of polarization (P) — at (I,) incidences *greater*, it deviates on the *same* side as P; results which *agree exactly with numerous and accurate observations of Fresnel, Arago, and Brewster.*

We have also the following results of this last formula:

While α has any finite value, when $i = 0$, $\beta = \alpha$, or the plane of polarization is unchanged.

When $(i+r) = 90°$, $\beta=0$, or at the angle of complete polarization P coincides with I.

When $i = 90°$, $\beta = \alpha$ again, or P has its original position.

If $\alpha=0$, h' sin. $\alpha=0$, and if at the same time $(i+r) = 90°$, then $k'=0$, or we also see that *at the polarizing angle an incident ray polarized in* I *will cease to give any reflected ray;* which agrees with the observation originally made by Malus.

From the same formulas another more curious inference was made by Fresnel as follows: — In passing out of a denser into a rarer medium, in general it is well known if $i = 90°$, sin. $i = \frac{1}{\mu}$

Consequently a ray making this incidence internally on the bounding surface will not be refracted out; and at incidences more oblique is experimentally found to be totally reflected internally: theoretically, the conservation of *vis viva* would require that the whole vibratory force, since none of it is expended on refraction, must be occupied in communicating vibrations internally, which can only produce internal waves or internal total reflexion.

Now at the critical incidence, in the formulas for h' and k', sin. $(i-r) = \cos. i$. sin. $(i+r) = \sin. i$ and tan. $(i-r) = \cot. i \tan. (i+r) = \tan. i'$; whence $h' = 1$ and $k = 1$, which accords with total reflexion.

At incidences greater than this the values become *imaginary;* and by introducing into them empirically certain terms[1] multiplied by $\sqrt{-1}$ Fresnel obtained in such cases an expression of the form,

$$(\cos. \theta + \sqrt{-1} \sin. \theta) \sin. \frac{2\pi}{\lambda}(vt - x)$$

[1] See Airy's Tract, Art. 153.

The interferences of rays have occupied so great a space in this biography that I cannot dispense with pointing out how they are connected with the two theories of light; how in the theory of emission I do not hesitate to say, if we admit no dependence between the motions of the different luminous molecules (and I know not what dependence we can establish between isolated projectiles), the fact, and above all the laws, of interference appear

And by the analogy of certain geometrical cases where the multiplication by $\sqrt{-1}$ indicates a line differing in angular position by 90°, he hazarded the inference that such an interpretation might hold good here, and that this expression would be equivalent to one of the form,

$$\cos.\ \theta \sin.\ \frac{2\pi}{\lambda}\ (vt-x) + \sin.\ \theta \sin.\ \frac{2\pi}{\lambda}\ (vt-x + 90^\circ)$$

which is trigonometrically the same as

$$\sin.\ \left(\frac{2\pi}{\lambda}\ (vt-x)\ +\ \theta\right)$$

This applying to the component in the plane of incidence, a similar expression would apply to that perpendicular to it,

$$\text{or } \sin.\ \left(\frac{2\pi}{\lambda}\ (vt-x) + \theta_{,}\right)$$

The difference of these expressions, or the relative retardation of the two sets of waves, will be $\theta - \theta_{,} = \delta$.

In general, δ having any value, and the plane of polarization being inclined at an angle α to the plane of incidence on the rhomb, the components are,

$$y = \sin.\ \alpha.\ \sin.\ \frac{2\pi}{\lambda}\ (vt-x + \delta) \tag{1.}$$

$$z = \cos.\ \alpha.\ \sin.\ \frac{2\pi}{\lambda}\ (vt-x) \tag{2.}$$

This then is precisely the same case as that considered in a former note: and exactly in the same way we obtain,

$$\frac{y^2}{\sin.\ ^2\alpha} + \frac{z^2}{\cos.\ ^2\alpha} - \frac{2yz\ \cos.\ \delta}{\cos.\ \alpha.\ \sin\ \alpha.} = \sin.\ ^2\delta.$$

The general equation to an *ellipse*. If $\delta = 90°$, the semi-axes are sin. α and cos. α, parallel and perpendicular to the plane of incidence. If $\alpha = 45°$ and δ variable, it is still an ellipse. If $\alpha = 45°$ and $\delta = 90°$, it becomes a circle. *Thus a ray polarized at an angle α, with the plane of incidence, after two internal reflexions in glass, emerges elliptically or circularly polarized, according to the above condition.*

From the empirical terms before mentioned, Fresnel derived expressions from which he calculated that for crown glass, where $\mu = 1{\cdot}51$, an internal incidence $i = 54°\ 37'$ would give $\delta = 45°$. Thus experimentally cutting a rhomb of such glass at that angle, so that the ray polarized at 45° to the plane of incidence, entering one face perpendicularly, might be reflected internally at that angle, and, passing to the opposite side, be reflected again internally at the same angle; after two reflexions it would emerge, consisting of two pencils polarized at right angles to each other, and having a difference of phase $\delta = 90°$, and would thus possess a circular polarization; or if the inclination was any other than 45° and δ differing from 90, the polarization would be elliptic of different degrees; all which conclusions are fully verified by experiments as before noticed.

wholly inexplicable. I will add besides, that none of the partisans of the system of emission have attempted in any published work to remove the difficulty, and it is not to be supposed that they had despised it.

As to the system of waves, the interferences are so natural a deduction from it, that we have some reason to be astonished that experimenters should have discovered them before theory had indicated them. To convince ourselves of this, it suffices to remark that a wave, in propagating itself through an elastic medium, communicates to the molecules of which it is composed an oscillatory motion, in virtue of which they displace themselves successively in two opposite directions: this being understood, it is evident that a series of waves will destroy completely the effect of another series, if at every point in the fluid the motion in one direction which the first wave produces alone, shall coincide with the motion in the opposite direction which would result from the sole action of the other wave. The molecules solicited at the same time by equal forces diametrically opposed, will then remain at rest, for as long a period as they would have freely oscillated if under the action of one wave alone. Motion has destroyed motion; now motion is light.

I will not push further this enumeration, because we can already judge on how many points the antagonists of the emission theory have been successful in their attacks. Experiments so numerous, so varied, so delicate, as those I have referred to, do not alone testify all the importance which the question seems to them to possess; they must be regarded further as a striking mark of respect towards the great man whose name, so to speak, has been identified with the theory which they think ought to be rejected. As to the theory of waves, the Newtonians have not done it the honour to discuss it with the same detail: it has seemed to them that a single objection was sufficient to annihilate it; and this objection they have drawn from the manner in which sound is propagated in air. If light, they say, is a vibration like the vibrations of sound, it will be transmitted in all directions: just as we hear the sound of a distant bell when we are separated from it by a screen which conceals it from our eyes, in the same way we ought to perceive the light of the sun behind every kind of opaque body. Such are the terms to which we must reduce the difficulty, for analogy does not permit us to say that light ought to extend itself behind screens without losing some of its intensity; since sound itself, as every one knows, does not penetrate obstacles without being enfeebled in a sensible degree. Thus, in speaking of the extension of light into the geometrical shadow of a body as an insurmountable difficulty, Newton and his adherents certainly did not suspect the answer which it would bring with it; yet this

answer is direct and simple. You maintain that the luminous vibrations ought to extend into the shadow, — *they do so.* You say that in the system of waves, the shadow of an opaque body can never be completely dark, — *it never is so.* It includes a number of rays which give rise to a multitude of curious phenomena, of which you may have some knowledge, since Grimaldi perceived them in part so long ago as before 1633.* Fresnel,— and here

* Among the earliest difficulties which seemed to attend the conception of the wave theory, was the consideration, which appeared so unanswerable, that on this principle there ought to be no *darkness:* light ought to spread equally into the shadow, and we ought to see round a corner.

It was the fertile principle of interference which was to supply the answer, as indeed had been long before hinted generally by Huyghens. The waves diverging from the *different parts* of a luminous source of any *sensible* magnitude interfere with and neutralise each other, except in the main direction, when alone they exactly concur; — a principle called "the mutual destruction of secondary waves." Young dwelt much at first on this objection; and afterwards, in a letter to Arago, he renews a similar expression of the difficulties he felt in another point of view:—"If light has so great a tendency to diverge into the path of neighbouring rays, and to interfere with them, as Huyghens supposed, I do not see how it escapes being totally extinguished in a very short space, even in the most transparent medium."—Peacock's Life, p. 140. But the principle just adverted to shows that the middle portion of the light coming from a point of any physical magnitude is not subject to those mutual interferences, and does not diverge, but is perpetually reinforced by the supply of fresh waves incessantly propagated from the original source. In these explanations Young at length expressed his full concurrence in a letter to Fresnel. The actual divergence of light into a shadow is demonstrated by the existence of the internal stripes. This, however, is an effect only produced to a very limited extent; and the general law of the "mutual destruction of secondary waves" in ordinary cases applies to produce the effect of destroying all apparent lateral divergence. There are, however, some cases where this cause operates less extensively (such, at least, would seem to be the case, and is the view upheld by some mathematicians); at all events, under certain conditions, the divergence is rendered very much more conspicuous, and reaches to a far greater distance from the edge. This appears to have been the case in a remarkable experiment, mentioned both by Newton and Hooke, and probably observed by each independently, but described, especially by Newton, in somewhat obscure terms (see Optics, book iii. part i. obs. 5. (Ed. 1721.) but more precisely by Hooke: see Posthumous Works, pp. 186. and 190. and plate 11. fig. 8. p. 155. Ed. 1705). Hooke ascribes it to a "deflexion of light differing both from reflexion and refraction, and seeming to depend on the unequal density of the constituent parts of the ray," &c. Newton enters on no theoretical considerations whatever, but mentions it only among those unfinished inquiries which, as he says, he had left imperfect and was unable to carry out.

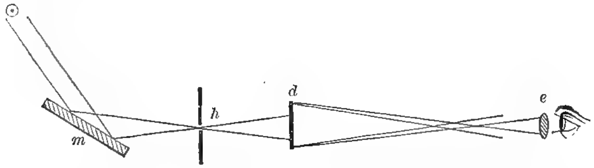
Both the fact, and all questions relating to it, seem to have been overlooked until, in reference to a somewhat similar case, M. Babinet supposed that under particular conditions the mutual interference of the secondary waves might be interrupted by stopping one of the interfering portions of light, and thus the other portion be rendered effective, and consequently diverging rays made visible. The author of this note, in relation to what appears a closely allied, if not identical phenomenon, the formation of a corona or ring of light round the

is incontestably one of the most important of his discoveries, —has shown how and under what circumstances this divergence of light takes place: he has further shown that in a complete wave which is freely propagated, the rays are only sensible in the directions which, prolonged, terminate in the luminous points, although in each of its successive positions the different parts of the primitive wave are in fact themselves the centres of disturbance, whence emanate new waves in all directions; but these oblique or secondary waves interfere with each other, and destroy each other entirely. There remain then only the normal waves: and thus the rectilinear propagation of light finds an explanation in the system of vibrations.

When the original wave is not entire, when it is broken or intercepted by the presence of an opaque body, the result of the interferences (which in this case play an important part) is not so simple to explain: the rays which go off obliquely from all parts of the wave not intercepted, do not necessarily destroy each other. In one part they conspire with the normal ray, and produce

dark disk of the moon in a total eclipse of the sun, tried some analogous experiments, and rendered the same kind of effect conspicuous and easy to be studied by an arrangement of this kind: —

The rays of the sun ⊙ are transmitted by reflexion from an inclined mirror (*m*) through a small hole (*h*) in a shutter, and in the diverging beam is placed an opaque circular disk (*d*) which intercepts the rays at a point where they have an area *considerably less* than its own diameter. From the edge of (*d*) rays are seen to diverge into its shadow and cross at successive points along the axis;



they are thus rendered visible by means of a small eyelens at (*e*) which presents the appearance of the shadow of the circular disk, having a multitude of rays converging inwards from its edge to its centre, where they form a point or small circle of great relative brightness. If, on the other hand, the disk (*d*) under the same conditions be viewed *directly* by the eye, without the lens, its shadow is seen relatively and uniformly dark, but surrounded by a bright luminous ring on its outside. The same appearance of the ring is also presented if, instead of the solar rays, we use the light of a flame placed at the principal focus of a lens inserted in a screen so as to send out a beam of parallel pencils intercepted in like manner by the disk. In this case, however, the converging rays cannot be seen. This apparently paradoxical effect has been supposed by some not sufficiently explained on M. Babinet's principle. The reader will find some observations on the subject, and its applications in the author's two papers in the Memoirs of the Royal Astronomical Society, vol. xvi., on Luminous Rings round Shadows, and in vol. xviii., on Irradiation. Some further remarks also will be found in his paper on Lord Brougham's Experiments, Phil. Mag. July, 1852.—*Translator.*

a brilliant light; in another these same rays destroy each other, and all light disappears. From the point where a ray is broken, its propagation is effected thenceforward according to special laws; the light which falls upon a screen is no longer uniform: it necessarily is composed of alternate stripes of brightness and darkness regularly placed. If the opaque intercepting body is not very large, the oblique waves which cross each other within its shadow produce, by their reciprocal action, stripes analogous to the former, but differently distributed.

I perceive that, without intending it, in following the theoretical speculations of Fresnel, I have mentioned the principal features of those curious phenomena of diffraction, which I have before cited under another point of view, to which Newton devoted one entire book of his *Optics*. Newton believed that he could not give any explanation of these phenomena (so difficult did they seem to him), except by admitting that a ray of light cannot pass close to a body without there undergoing a sinuous movement like that of an eel. In the explanations of Fresnel this strange supposition is superfluous.

The opaque body which seems to be the original cause of the diffracted bands does not act at all on the rays, either by attraction or by repulsion; it simply intercepts a part of the principal wave. It stops in the ratio of their breadth a great number of oblique rays, which, but for this interruption, would have gone into certain parts of space to mix with other rays, and to interfere more or less with them.

Thus it is no longer surprising that, as observation has proved, the resulting effect is independent of the nature and mass of the body. The periods of maximum and minimum of the light, as well without as within the shadow, are directly deducible from the theory of Fresnel with a degree of precision of which hitherto, perhaps, no branch of physical science had afforded so striking an example. Thus, whatever reserve it may be prudent to impose on ourselves when we run the risk of speaking of the labours of our successors, I would almost venture to affirm that, with regard to diffraction, they will add nothing essential to the discoveries with which Fresnel has enriched the science. Theories are, in general, only methods, more or less happy, of linking together a certain number of facts *already known*. But when all the *new consequences* which we can deduce from them are found to agree with experience, they claim a higher importance. This kind of success has not been wanting to Fresnel. His formulas of diffraction include, by implication, a very strange result, which he had not perceived. One of our colleagues*—I shall have no need

* Poisson.

to mention his name, if I say that he has been placed long since among the greatest geometers of this age, as well by a multitude of important labours in pure analysis, as by the most happy applications to the system of the world, and to physics, — perceived at a glance the consequence of which I have spoken; he showed that, in admitting the formulas of Fresnel, the centre of the shadow of an opaque and circular screen ought to be as bright as if the screen did not exist. This consequence, apparently so paradoxical, was subjected to trial by direct experiment, and observation has perfectly confirmed the result of calculation.

In the long and difficult discussion to which the nature of light has given birth, and of which I have just traced the history, the task of the physicists has been nearly fulfilled; as to that of the mathematicians, it unhappily still offers some deficiencies to be filled up. I would venture then, if I had the right, to adjure that great geometer to whom optical science owes the important result just mentioned, to try whether the half empirical formulas by which Fresnel has attempted to express the intensities of light reflected under all angles and for all kinds of surfaces, may not be found deducible also from the general equations of motion of elastic fluids. It remains, above all, to explain how the different undulations can undergo unequal deviations at the bounding surfaces of transparent bodies.

LIGHT-HOUSES.

In an academy of sciences, if it properly appreciate its functions, the author of a discovery is never exposed to the discouraging question so often addressed to him in the world, of *cui bono?* Here every one comprehends that the animal life ought not to be the sole occupation of man; that the cultivation of his intellect,— that an attentive study of this infinite variety of animated beings, and inert matter, with which he is surrounded, forms the most beautiful portion of his destined pursuits.

But besides, even if we were desirous to find nothing in the sciences but the means of facilitating the reproduction of substances for food,—of weaving with more or less economy and perfection the different fabrics which serve for clothing,—of constructing with elegance and solidity the convenient habitations in which we escape the vicissitudes of the seasons,—of extracting from the bowels of the earth so many metals and combustible matter, which are necessary for the arts of life,—of annihilating a hundred material obstacles which oppose themselves to the inter-communication of inhabitants of the same continent, of the same kingdom, even of the same city,—of extracting and preparing the medicaments proper for combating the numerous disorders with which

our organs are incessantly threatened,—the question of *cui bono?* will be found completely announced. Natural phenomena have innumerable points of connexion with each other, often hidden, the discovery of which one age bequeaths to another. At the moment when these relations are discovered, important applications rise up, as if by enchantment, out of experiments which, until then, would seem likely to remain for ever among the number of abstract speculations. A fact which no direct utility had as yet recommended to the attention of the public becomes, perhaps, the step on which a man of genius supports himself to climb up to those primary truths which change the whole face of science, whether for creation of some economical moving power, which all manufacturing arts will henceforth adopt, and of which not the least merit is that of delivering thousands of operatives from overwhelming toils which assimilated them with the brutes, ruined their health, and brought them to a premature death. If to fortify these reflections examples may be thought necessary, I should feel no other embarrassment than that of too wide a choice. But here there is no necessity to enter on such details; for to all the theoretical researches already mentioned, Fresnel has added an important labour, having an immediate practical application, which will certainly place his name among those of the benefactors of the human race. This work, every one knows, had for its object the improvement of *light-houses*. I will proceed to trace the outline of its progress, and shall thus have finished the sketch which I proposed to offer you of the brilliant scientific career of our late colleague.

Persons unacquainted with nautical matters are usually seized with a sort of fear when the vessel which carries them, at a distance from continents or islands, has no other witness of its progress than the stars and the waves. A view of any coast the most barren, the most rocky, the most inhospitable, dissipates, as if by enchantment, those undefined fears which their absolutely isolated position had inspired, while, to the experienced navigator, it is near the land alone that the dangers are seen to commence.

Such danger occurs in ports into which no prudent sailor would enter without a pilot; it occurs where, even with this help, no one would risk attempting to penetrate at night; we easily see, then, how indispensable it is, if we would avoid irreparable accidents, that after sunset signals of flame, easily visible, should indicate on all sides the proximity of land. It is necessary moreover that every ship should perceive the signal far enough off for it to find, in evolutions often sufficiently difficult, the means of keeping itself at some distance from the shore until the moment when day shall appear. It is not less desirable that the different lights which we kindle along a certain extent of coast should not be confounded with each other; and that at first sight of these hospitable signals

the navigator, who by an unfavourable sky has been for some days deprived of the means of directing his course, should know, for example, on returning from America, whether he is about to enter the Gironde, the Loire, or the harbour of Brest.

On account of the roundness of the earth, the range of a light-house depends on its height. In this respect men have always obtained without difficulty the range which the wants of navigation demanded: it was a simple question of expense; every one knows, for instance, that the great edifice with which the famous architect Sostrates of Cnidus adorned the harbour of Alexandria, nearly 300 years before our æra, and most of the light-houses constructed by the Romans, were of considerably greater height than the most celebrated modern towers. But in an optical point of view, these light-houses were but little remarkable; the feeble rays which proceeded from fires of wood or of coal, lighted in the open air on their summits, could never penetrate the thick vapours which in all climates obscure the lower regions of the atmosphere.

Nevertheless, as to the intensity of light, the modern light-houses were but little superior to the ancient. The first important amelioration which they received, dates from the double-current lamp of Argand; that admirable invention which would be much better appreciated, if, while our museums include works of the period of the decline of art in a purely historical point of view, the repositories of industrial science presented successively to public inspection the various means of illumination, so dull, so bad, so ill-suited, so nauseous, which were employed only fifty years ago, by the side of those elegant lamps whose pure and brilliant light rivals that of a summer day.

Four or five Argand lamps united, would give without doubt as much light as the large fires which the Romans used, with so much trouble, on the lofty towers of Alexandria of Puzzuoli, or of Ravenna; but in combining these lamps with reflecting mirrors, their natural effects may be prodigiously increased. The principle of this last invention ought to arrest our attention for an instant, because it will enable us rightly to appreciate the value of Fresnel's labour.

The light of a burning body expands uniformly in all directions, — one part falls on the ground, and is lost; another portion ascends, and is dissipated in space; the sailor whose route we wish to enlighten, profits only by those rays which are emitted horizontally, or nearly so, from the lamp across the sea; all the rays, even those which are horizontal, directed towards the land, have only been produced to be entirely wasted.

This horizontal beam of rays not only forms a very small part of the total light; it has also the serious inconvenience of diminishing in intensity as it diverges, and of not extending itself to a

distance without being sensibly enfeebled. To destroy this unfortunate loss of light,— to profit by *all* the light which the lamp emits,—was the twofold problem which remained to be resolved in order to extend the range, and thus the utility, of light-houses. Concave metallic mirrors, called *parabolic reflectors,* have furnished a satisfactory solution.

When the lamp is placed at the focus of such a mirror, all the rays which emanate from it are brought, by the reflexion they undergo against its sides, into a common direction; their original divergence is destroyed; they form, as they issue from the apparatus, a cylinder of light parallel to the axis of the mirror. This beam is transmitted to the greatest distances with the same brightness, except that the atmosphere absorbs a small part of it.

Before proceeding further, let us stop to observe that this construction is not without an inconvenience. We thus indeed easily bring to bear on the horizon of the sea a multitude of rays which would otherwise have been lost on the ground, in space above, or on the side towards the land; and we overcome the divergence of those rays which would naturally be directed towards the navigator. But the cylinder of reflected rays can have no greater breadth than that of the mirror; the space which it illuminates has precisely the same breadth at all distances; unless indeed we employed many similar mirrors, pointed different ways, and even then the horizon would include many large spaces completely dark, in which the pilot would perceive no signal. This great evil is overcome by giving, by means of clockwork, an uniform motion of rotation to the reflector. The luminous beam issuing from this mirror is then successively directed to all points of the horizon; every ship sees the light at one instant appear, and at another disappear; and if in a great length of coast, as for example from Brest to Bayonne, there do not exist any two light-houses with the same period of rotation, all the signals are, so to speak, individualised. According to the interval which elapses between two appearances or two eclipses of the light, the navigator always knows what point of the coast is in view; he finds himself no longer liable to mistake the light-house for a planet or star of the first magnitude near to its rising or setting, or even for those accidental fires, kindled on the coast by fishermen, woodcutters, or charcoal burners,—fatal mistakes which have often been the cause of deplorable shipwrecks.

A transparent lens brings to parallelism all the luminous rays which traverse it, whatever might be their original degree of divergence, provided the point from which the rays diverge be coincident with that point belonging to the lens which we call its focus. Glass lenses, then, may be substituted for mirrors, and in fact a light-house with lenses has been long ago executed in

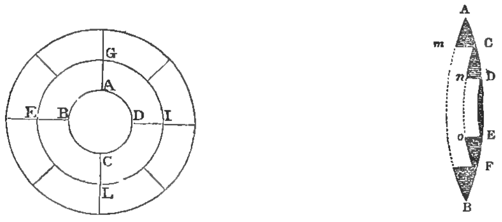
England under the idea, at first sight very plausible, that it would be much more brilliant than light-houses with reflectors. Yet it was found in practice that mirrors, notwithstanding the great loss of light which they produce at their surface in the act of reflexion, direct to the horizon a more intense beam of light. Lenses were therefore abandoned.

The unknown author of this abortive attempt proceeded at hazard. In occupying himself with the same problem, Fresnel, with his habitual penetration, perceived at the first glance where the difficulty lay. He saw that the lenticular light-houses could only become superior to those with reflectors, by increasing considerably the intensity of the flame which supplied the illumination; or by giving to the lenses enormous dimensions which seemed to surpass all that any ordinary work could accomplish. He observed also that the lenses must have a very short focal length, that in making them according to the usual forms, they had too great a thickness, too small a transparency, and that their weights were considerable, and pressed too much on the machinery for making them rotate, so as speedily to bring on its destruction.

To avoid this excessive thickness of the ordinary lenses, their enormous weight, and want of transparency, which were its consequences, they were replaced by others of a peculiar form, which Buffon had imagined for another purpose, and which he called *lenses by steps.* (*Lentilles à échelons.*)* It is possible at the present day to construct the largest lenses of this kind, although we do not yet know how to fabricate thick masses of glass free from defects. It suffices to compose them of a number of distinct small pieces; a plan proposed by Condorcet.

I can here affirm that, at the moment when the idea of these lenses "by steps" occurred to the mind of Fresnel, he had not the

* The nature of these lenses *à échelons* will be understood at once from the annexed sketch, where this construction is represented in front view and in



section. The effect of one continuous lens is made up by a combination of separate pieces, instead of one large lens as indicated by the dotted outline.—See Brewster's *Optics*, Cab. Cyclop. p. 322.

least knowledge of the previous projects of Buffon and Condorce But assertions of this kind are interesting only to the author i regard to his own claims, they have no value for the public. I: its eyes there is not,—I will say more, there ought not to be mor than one inventor,—he who first makes public the discovery. Afte so large a concession, it may at least be allowed me to remark tha in 1820 there did not exist a single lens of this construction i the physical cabinets, and that besides, up to that time, lenses ha only been regarded as the means of producing great effects o *heat;* that it was Fresnel who created methods to construct then with exactness and economy; that it was he, and he alone, wh even imagined the application of them to *light-houses.* Thi application, however (as I have just pointed out), could never hav led to any useful result if it had not been combined with suitabl modifications of the lamp; if the illuminating power of flame ha not been greatly augmented. This important part of the systen required special studies, numerous and delicate experiments Fresnel and one of his friends (Arago) devoted themselves to th inquiry with ardour; and their common labour led to the con struction of a lamp with many concentric wicks, whose brillianc was twenty-five times that of the best lamps, with only a doubl current.

In the glass lenses imagined by Fresnel, each lens sent succes sively to all parts of the horizon a light equivalent to that o 3,000 or 4,000 Argand lamps united; that is, eight times tha produced by the beautiful silvered parabolic reflectors of whic our neighbours make use; it is also equivalent to the light whic we should obtain by uniting in one the third part of the tota quantity of the gas-lights which illuminate the streets, the shops and the theatres of Paris. Such a result does not seem devoid o importance, if we remark that it is obtained with a single lamp In perceiving such powerful effects, the Government took care t authorise Fresnel to cause to be constructed one of his instruments and selected the lofty Tower of Cordouan, at the mouth of th Gironde, as the point where it should be placed. The new light house was at length constructed in the month of July, 1823.

The light-house of Fresnel has since had for judges, durin seven consecutive years, the multitude of mariners of all countrie who frequent the Gulf of Gascony. It was also studie assiduously at the place by skilful engineers, who came expressl from the north of Scotland with a special mission from the Britis Government. I shall here be the interpreter of the opinions bot of the one and the other, when I affirm that France, since ther the important invention of revolving lights had its origir possesses, thanks to the labours of our scientific colleague, th

most beautiful light-houses in the world. It is always glorious to march at the head of the sciences; but we experience above all a lively satisfaction in claiming the first rank for our country, when the question relates to one of those happy applications in which all nations are called upon to take an equal part, and of which humanity will never have occasion to complain.

There exist at present on the ocean and on the Mediterranean twelve light-houses, more or less powerful, constructed on the principles of Fresnel. To complete the general system of lighting our coasts, thirty new light-houses appear still necessary. Everything induces us to hope that these important works will be promptly executed, and that we shall deviate in the least possible degree from the happy direction given to this branch of the public service by our colleague. Routine and prejudice will here be without power, since the parties interested who are the true judges, the mariners of all nations, have unanimously proclaimed the superiority of the new system. No one can allege pretexts of economy, for to produce equal effect, the lenticular light-houses do not require so much oil as those of the old construction; are of a much less expensive kind to keep up, and procure definitively to the State an annual economy of about half a million. This beautiful invention, then, ought to prosper at least, if since the death of Fresnel it did not fall into the hands of those persons, strangers to the subject, who think themselves fit for all employments, although, under different states of public affairs, they have had no other places of study than the antichambers of ministers. Candidates, if I am rightly informed, were not wanting; but happily, this time, intrigue yielded to merit, and the chief superintendence of the light-houses was entrusted to the younger brother of Fresnel, like himself a former pupil of the Ecole Polytechnique, like himself an engineer of the "ponts et chausées," — skilful, zealous, and conscientious.

Under his inspection, the construction and the disposition of great lenses "in steps" has received important improvements, and the public will not have to fear that any negligence will deprive these beautiful instruments of any part of their power. Such inheritances of national glory will surely never be allowed to suffer neglect.

LIFE AND CHARACTER OF FRESNEL.—HIS DEATH.

The numerous discoveries which I have just described were all made in the short interval between 1815 and 1826, without occasioning any neglect of the duties confided to Fresnel, either as engineer of the pavements of Paris, or as secretary of the com-

mission of light-houses. But our colleague, at the same time entirely withdrew from the temptations to idleness, which abound more in Paris than any other city, and which those who yield themselves to them call the duties of society, in order to appease their consciences, and to explain to themselves how their time is so ill employed. A life in the study, a life altogether intellectual, however, was but ill suited to the frail constitution of Fresnel. However, the anxious cares of his estimable family were abundantly bestowed on him;—the thoroughly contented disposition of this simple-minded man, than whom no one ever better deserved the title, reacted powerfully in preserving his health;—and lastly, his extreme temperance led to the hope that he might be long spared to the sciences.

The emoluments of the two offices held by Fresnel, that of engineer and academician, would have amply sufficed for his moderate desires, if the craving for scientific research had not been with him a second nature. The construction and purchase of those delicate instruments, without which, at the present day, we cannot produce anything exact in physics, absorbed every year a considerable part of his fortune. He, therefore, was anxious to create new resources. The situation, so very moderately remunerated, of temporary examiner of the pupils at the Ecole Polytechnique offered itself; Fresnel obtained it: but his friends were not slow to perceive that he had presumed too much on the powers of his constitution; that the ardour with which he fulfilled his new duties and the anxieties he felt,—in fact unduly exaggerated,—in classing the candidates in the order of merit, seriously affected his health, already so precarious; and yet, how could they advise a resignation, of which the inevitable result would be the abandonment of many glorious labours? Under these circumstances, one of the most desirable scientific offices, among all those of which the government has the disposal, that of examiner of the pupils in navigation, became vacant. This office requires only moderate labour. The annual journey which it involves was, in the eyes of his medical advisers, a reason why it should be more desirable that Fresnel should obtain it. He determined, therefore, to become a candidate; as every one would believe, there is no impropriety in asking for an employment, for which long studies peculiarly qualify a person, and which he could conscientiously fulfil. Literary men suppose that after undertaking toilsome labours they can, without reproach, aspire to the enjoyment in their old age of that independence which the most inconsiderable artisan in Paris is sure of obtaining one day, however slight may be his labours or inferior his rank. No one has ever maintained that there is not both propriety and advantage in

every case in choosing the most worthy. The glory which such men as Lagrange, Laplace, Legendre, reflected on the board of longitude and on the Academy, seemed to associate itself with the eminent services which, under other titles, these illustrious geometers had rendered to the Ecole Polytechnique. In the public courses, the pupils claim that the professors should be earnest, lucid, and methodical: but it is no concern of theirs to inquire whether other audiences in other establishments receive instruction from the same men. The sciences will not appear an idle superfluity; and we may admit that Papin, in inventing the steam-engine; Pascal, in pointing out the principle of the hydraulic press; Lebon, in imagining lighting by gas; Berthollet, in inventing bleaching by chlorine; Leblanc, in teaching us to extract from sea-water the soda which formerly had to be imported at high prices; have nobly paid to society the debt of science.

If we ought to believe some persons, whose intentions I would rather commend than their enlightenment, I should have to enumerate a long series of prejudices, and should have to defend the author of so many beautiful discoveries, the originator of a new system of light-houses, the man of science whose name navigators will eternally bless, from the charge of having desired, by the union of two offices, to procure for himself an annual life-income of 12,000 francs, of which the greatest part would certainly have been devoted to the expenses of new researches. The defence of our colleague would, without doubt, be an easy task; but I may omit it: Fresnel did not obtain the employment he sought, and that from causes which I would willingly pass over in silence, if they were not such as to give me occasion for showing that men of letters,—whose character there have recently been attempts to dishonour, by representing them as harpies rushing without rule or moderation to prey upon the public purse,—know well how to renounce nobly the most desirable offices, even those which they might claim as a sacred debt, as soon as their dignity would become compromised.

I have already mentioned how much the duties of examiner at the Ecole Polytechnique endangered the health of Fresnel; how desirable it became that his wish for a less laborious situation should be attained. The incontestable superiority of his scientific claims, the withdrawal of all competitors, the behaviour of one of our honourable colleagues, one of the first geometers of the age; and lastly, the active conduct of M. Becquey, who, on every occasion, treated Fresnel with the kindness of a father, had smoothed over many obstacles. The minister on whose decision the appointment depended had himself, during his youth, been occupied with the study of the sciences in a distinguished

degree; he had even kept up the taste for them: he desired to see our colleague, and from that moment his nomination seemed sure: for the reserved manner of Fresnel, the sweetness of his character, the unaffected modesty of his language, conciliated instantly the goodwill even of those who did not understand his works; but, alas, in the train of civil discords, to how many mistakes are we not exposed, if we proceed to judge of that which will be from that which ought to be. How many little circumstances, paltry interests, heterogeneous elements, come in and mix themselves with affairs the most simple, and prevail over rights the most incontestable. For my own part, I cannot say on what occasion the Minister of the Interior, addressing himself to the royalist volunteer of the Drôme, put the following question, clearly intimating that his nomination depended on the answer he gave: "Sir, are you truly on our side?" "If I understand rightly, Monseigneur, I should answer that there exists no one more devoted than myself to the august family of our kings, and to the wise institutions which France owes to it." "All this, sir, is too vague: we shall understand one another better by using plain terms. If you were a deputy, by the side of which member would you sit?" "Monseigneur," replied Fresnel, without hesitation, "by the side of Camille Jordan, if I were worthy." "Many thanks for your frankness," replied the minister. The next day an unknown individual was named examiner of the marine.

Fresnel received this repulse without a word of complaint. In his mind, the personal question was entirely effaced in comparison with the pain he felt in seeing, after thirty years of debates and troubles, political passions still so little subdued. When a minister, whose private qualities might claim the homage of good men of all parties, considered himself obliged to ask a scientific examiner, not for proofs of incorruptibility, of zeal, or of knowledge, but for an assurance that, if by chance he should ever happen to become a deputy, he would not determine to sit at the side of Camille Jordan, a good citizen could not but fear that our political future was not to be exempt from storms.

The body of instructors of the Ecole Polytechnique, under all régimes, has suffered little from political influences. There the examiner and the professor must daily discharge their duties in person; there, under the eyes of a nursery of skilful hearers, and in some slight degree inclined to malice, inaccurate refinements, false calculations, bad experiments in chemistry or physics would in vain seek refuge under the shelter of the opinions of the day. Fresnel might then hope that, notwithstanding his recent profession of faith, they would not deprive him of the place of temporary examiner. Besides, this office is extremely laborious, and

experience has sufficiently shown, that sinecures are the places sought after with more especial ardour. Fresnel then continued his former functions: but at the close of the examination of 1824, an attack of hemoptysis forced him to retire from his labours, and caused the most serious alarm to his friends. From this moment our unfortunate colleague was obliged to abandon every scientific research which required close attention, and to devote solely to the business of the light-houses the few moments of relief which his malady left him. The most tender and marked attentions soon became powerless against the rapid progress of the disease. It was then resolved to try the effects of country air: alas! but a too evident indication of the little hope entertained by the skilful physician in whom Fresnel confided. However, not to distress his family, our unfortunate colleague affected to entertain hope, and at the beginning of June, 1827, he was removed to Ville d'Avray. There he saw the approach of death with the calmness and resignation of a man whose whole conduct had been without reproach. A young engineer of high distinction, M. Duleau, found, in the lively friendship which united him to our colleague, an irresistible impulse to take part in the melancholy kind offices of which he was the object; and he also established himself at Ville d'Avray. M. Duleau was the first who informed us how little Fresnel was under any delusion as to his condition. "I could have wished," he exclaimed sometimes (when the presence of a mother and a brother, who were agitated by poignant disquietude, did not impose upon him a reserve which his tender feelings for them would not infringe), "I could have wished to live longer, because I perceive that there are in the inexhaustible range of science, a great number of questions of public utility, of which, perhaps, I might have had the happiness of finding the solution." Fresnel was still in the country when the Royal Society of London charged me with the office of presenting to him the Rumford Medal. His powers, then almost exhausted, scarcely permitted him to cast a glance of his eye over this testimony, so rarely bestowed, of the estimation of that illustrious society. All his thoughts were directed towards his approaching end: all were concentrated on that object. "I thank you," he said to me, in a feeble voice, "for having undertaken this mission. I guess how much it must have cost you, for you have perceived, is it not so? that the most beautiful crown is worth little when it is only to be deposited on the tomb of a friend!"

Alas! these melancholy anticipations were not long in being accomplished. Eight days more had hardly elapsed when our country lost one of its most virtuous citizens; the Academy one of its most illustrious members; and the scientific world, a genius of the highest order.

Newton, on learning the premature death of Cotes, a young geometer whose first labours had led to great expectations, pronounced those words, so simple, so expressive, that the history of science has treasured them up: "If Cotes had lived we should have known something!" From the mouth of Newton this short eulogy might pass without comment; it belongs to genius to pronounce such sentences, and we shall always believe its word. For myself, Gentlemen, devoid of all such authority I have felt myself bound laboriously to go through so many details, not to affirm, but to prove to you, that we know some things although Fresnel lived so short a time.

THOMAS YOUNG.

A BIOGRAPHY READ AT A PUBLIC SITTING OF THE ACADEMY OF SCIENCES, THE 26TH OF NOVEMBER, 1832.

GENTLEMEN, — It seems as if death, who is incessantly thinning our ranks, directed his stroke with a fatal predilection, against that class of our body so limited in number, our foreign associates. In a short space of time the Academy has lost from the list of its members, Herschel, whose bold ideas on the structure of the universe have acquired every year more of probability; Piazzi, who on the first day of the present century presented our solar system with a new planet; Watt, who, if not the inventor of the steam-engine, the inventor having been a Frenchman*, was at least the creator of so many admirable contrivances, by the aid of which the little instrument of Papin has become the most ingenious, the most useful, the most powerful means of applying industry; Volta, who has been immortalised by his electric pile; Davy, equally celebrated for the decomposition of the alkalies and for the invaluable safety lamp of the miner; Wollaston, whom the English called the pope, because he never proved fallible in any of his numerous experiments, or of his subtile theoretical speculations; Jenner, lastly, whose discovery I have no need to extol in the presence of fathers of families. To pay to such of its distinguished ornaments the legitimate tribute of the regret, of the admiration, and the gratitude of all men devoted to study, is one

* This is not the place to enter on the controversy respecting the invention of the steam-engine. It may, however be remarked, that we may be well content to allow it to remain a question of *degree*. Every tea-kettle is a steam-engine. A very slight and obvious contrivance will enable steam to raise a piston. Let any one define what they mean precisely by the term steam-engine, and the question of priority of invention will be easily settled. — *Translator*.

of the principal duties which the Academy imposes on those whom it invests with the responsible honour of speaking in its name in these solemn meetings. To pay this grand debt with the least possible delay, seems an obligation not less imperative. Gentlemen, the native academician always leaves behind him, among the colleagues with whom he has been united by the election of the Academy, many confidants of his secret thoughts, of the origin and course of his researches, of the vicissitudes which he has gone through. The foreign associate on the contrary resides far away from us; he rarely joins in our meetings; we know nothing of his life, his habits, his character, unless from the reports of travellers. When several years have passed over such fugitive documents, if we still find any traces of them, we cannot reckon on their accuracy. Literary intelligence which has not found a record in print is a sort of coin, the circulation of which alters at the same time the impression, the weight, and the inscription.

These reflections tend to show why the names of such men as Herschel, Davy, or Volta ought to be mentioned in our assemblies before those of many celebrated academicians whom death has snatched from our more immediate circle. Moreover, I hope that after what I shall be able to adduce, even in a few minutes, no one will be able to deny that the man of universal science whose life I am about to describe, and whose labours I shall analyse, has some real claims to preference.

BIRTH OF YOUNG. — HIS CHILDHOOD. — FIRST ENTRANCE ON HIS SCIENTIFIC CAREER.

Thomas Young was born at Milverton in the county of Somerset, June 13, 1773, of parents who belonged to the Society of Friends. He passed his earliest years at the house of his maternal grandfather, Mr. Robert Davies, of Minehead, whom the active business of commerce had not been able to divert from the cultivation of classical literature. Young could read fluently at the age of two years. His memory was extraordinary. In the intervals of his attendance at the house of a village schoolmistress in the neighbourhood of Minehead, at four years old, he had learned by heart a number of English authors, and even several Latin poems, which he could repeat from beginning to end, although he did not understand a word of the language. The example of Young, like many others of celebrity recorded by biographers, may then contribute to keep up the common prepossession of so many good fathers of families, who see in certain lessons according as they may be recited without faults, on the one hand, or are badly learnt on the other, infallible indications of an eternal mediocrity in the

one case, or the beginning of a glorious career in the other. It would indeed be far from our object if these historical notices should tend to strengthen such prejudices. Thus, without wishing to weaken the vivid and pure emotions which every year the distribution of prizes excites, we may remind some, in order that they may not abandon themselves to dreams which they will not realise, and others, in order to fortify them against discouragement, that Picus de Mirandola, the phœnix of learners of all ages and countries, became in mature age an insignificant writer; that Newton — that powerful intellect of whom Voltaire, in some well known lines, asks the angels whether they are not jealous, — the great Newton, we observe, made but indifferent progress in the classes of his school; that study had for him no attractions; that the first time he felt the wish to labour it was merely to take the place of a turbulent schoolfellow, who, by reason of his rank in the school was seated on a form above him and annoyed him by kicks; that at the age of twenty-two he was a candidate for a fellowship at Cambridge, and was beaten by one Robert Uvedale, whose name, but for this circumstance, would have remained to this day perfectly unknown: that Fontenelle, lastly, was more ingenious than exact when he applied to Newton the words of Lucan, "It is not given to men to see the Nile feeble and at its source."

At the age of six years, Young entered under a teacher at Bristol*, whose mediocrity was a fortunate circumstance for him. This, Gentlemen, is no paradox: the pupil, not being able to accommodate himself to the slow and limited steps which his master took, became his own instructor. It is thus that those brilliant qualities developed themselves which too much aid would certainly have enervated.

Young was only eight years of age, when chance, whose influence in the events of man's life is more considerable than our vanity often allows us to admit, took him from studies exclusively literary, and revealed his real vocation. A surveyor of much merit in the neighbourhood took a great fancy for him: he took him out into the country sometimes on holidays, and permitted him to amuse himself with his instruments of surveying and natural philosophy. The operations, by whose aid the young scholar saw the distances and elevations of inaccessible objects determined, powerfully struck his imagination. But soon several

* The master, whose name was King, at first kept school at Stapleton, and thence removed to Townend, both near Bristol. Young's acquaintance with the surveyor commenced after he quitted that school. See Peacock's *Life*, p. 5. —*Translator.*

chapters of a mathematical dictionary made all that seemed mysterious in the matter disappear. From this moment, in his Sunday excursions, the quadrant took the place of the kite. In the evening, by way of amusement, the engineering novice calculated the heights measured in the morning.

From the age of nine to fourteen, Young went to a school at Compton in Dorsetshire, kept by Mr. Thomson, whose memory he always cherished. During these five years all the pupils of the school were occupied exclusively, according to the practice of English Schools, in a minute study of the principal writers of Greece and Rome.* Young continually maintained his place at the head of his class: and yet he learned at the same time French, Italian, Hebrew, Persian, and Arabic: French and Italian, from the chance object of satisfying the curiosity of a school-fellow who possessed some works printed at Paris, of which he was desirous to know the contents: — Hebrew, in order to read the Old Testament in the original: Persian and Arabic, with the view of deciding a question started at table, whether there were as marked differences between the Oriental languages as between those of Europe?

I perceive the necessity of mentioning that I write from authentic documents, before I add that during what might appear so fabulous a progress in languages, Young, during his walks at Compton, was seized with a violent passion for botany: and that being destitute of the means of magnifying objects of which naturalists make use when they wish to examine the delicate parts of plants, he undertook to construct a microscope himself, without any other guide than a description of the instrument in a work by Benjamin Martin: that to arrive at this difficult result it was necessary to acquire some skill in the art of turning: that the algebraic formulas of the optician having presented to him symbols of which he had no idea, (those of *fluxions*,) he was for a moment in great perplexity; but not being willing at last to give up the enlargement of his pistils and stamens, he found it more simple to learn the differential calculus, in order to comprehend the unlucky formula, than to send to the neighbouring town to buy a microscope. The ardent activity of the juvenile Young had led him to exertions beyond the strength of his constitution. At the age of fourteen his health was sadly altered. Various indications excited fears of a disease of the lungs; but these

* It would appear from Young's own account, that a far more liberal system was really pursued in this school. Also, the praises of the usher, Josiah Jeffery, should never be omitted, who initiated Young at leisure hours into a variety of experimental and practical subjects, which contributed materially to his future success. See Peacock's *Life*, p. 6. — *Translator.*

menacing symptoms at length yielded to the prescriptions of art, and the anxious cares of which this malady made him the object on the part of all his relations.

It is rare among our neighbours on the other side of the Channel * that a rich person, entrusting his son to the care of a private instructor, does not seek for him a fellow-pupil of the same age among those who have been remarkable for their success. It was in this capacity that Young became, in 1787, the fellow-pupil of the grandson of Mr. David Barclay, of Youngsbury, in Hertfordshire. On the day of his first appearance there, Mr. Barclay, who doubtless felt the right of showing himself somewhat exacting with a scholar of fourteen years of age, gave him several phrases to copy, with the view of ascertaining his skill in penmanship. Young, perhaps somewhat humiliated by this kind of trial, demanded, in order to satisfy him, permission to retire to another room; this absence being prolonged beyond the time which the transcription would have required, Mr. Barclay began to joke on the want of dexterity he must evince, when at length he re-entered the room. The copy was remarkably beautiful; no writing master could have executed it better: as to the delay, there was no longer any need to speak of it, for "the little quaker," † as Mr. Barclay called him, had not been content to transcribe the English phrases set him; he had also translated them into nine different languages.

The preceptor, or as they call him on the other side of the Channel, the *tutor*, who had to direct the two scholars at Youngsbury was a young man of much distinction, at that time entirely occupied in perfecting himself in the knowledge of the ancient languages; he was the future author ‡ of the Calligraphia Græca. He was not long, however, in perceiving the immense superiority of one of his pupils, and he recognised, with praiseworthy modesty, that in their common studies the true *tutor* was not always he who bore that title. At this period Young drew up, continually referring to the original sources, a detailed analysis of the numerous systems of philosophy which were professed in the different schools of Greece.§ His friends spoke of this work with the most lively admiration. I know not whether the public is destined ever to see it. At all events it was not without influence on the life of its author, for in giving himself up to an attentive

* The reader will of course make due allowance in this and many other passages for the ideas of a foreigner as to English habits. The anecdote of Young's penmanship which follows, is differently given by Dr. Peacock, p. 12. —*Translator*.

† This seems improbable, as Mr. Barclay's family were of the same sect.—*Translator*.

‡ Mr. Hodgkin.

§ This work is not mentioned by Dr. Peacock.— *Translator*.

and minute examination of the singularities (to use a mild term) with which the conceptions of the Greek philosophers teemed, Young perceived the attachment which he retained to the principles of the sect in which he was born became weakened. However, he did not separate entirely from it till some years afterwards, during his sojourn in Edinburgh.

The little studious colony at Youngsbury quitted the country during some months in the winter, to reside in London. During one of these excursions Young met with a teacher worthy of him. He was initiated into chemistry by Dr. Higgins*, whose name I can the less dispense with mentioning since, in spite of his earnest and frequent remonstrances, there was an obstinate disinclination to acknowledge the share which legitimately belonged to him in the establishment of the theory of definite proportions, one of the most valuable discoveries of modern chemistry.

Dr. Brocklesby, the maternal uncle of Young, one of the most popular physicians in London at the time, justly confident of the distinguished success of the young scholar, communicated occasionally his productions to men of science and literature, and to men of the world, whose approbation might have greatly flattered his vanity. Young thus found himself at an early period in personal relation with those celebrated men Burke and Wyndham, of the House of Commons, and the Duke of Richmond. The last nobleman, then Master of the Ordnance, offered him the place of private secretary. The two other statesmen, although they wished him also to follow a career connected with the public administration, yet advised him first to go through a course of law at Cambridge.† With such powerful patrons Young might reckon on one of those lucrative offices which persons in power are not slow to bestow on those who will spare them all study and application, and daily furnish them with the means of shining at the court, the council, the senate, without compromising their vanity by committing any indiscretion. Young happily had a consciousness of his powers; he perceived in himself the germ of those brilliant discoveries which have since adorned his name: he preferred the laborious, but independent, career of the man of letters, to the golden chains which they exhibited so temptingly to his eyes. Honour be to him for such a determination! May his

* The share borne by Dr. Higgins in the suggestion or discovery of the atomic theory has been variously estimated. For an apparently perfectly fair view of the case, the reader is referred to Dr. Daubeny's *Atomic Theory*, p. 33. — *Translator.*

† "Mr. Wyndham advised him not to accept the appointment, and recommended him rather to proceed to Cambridge, and study the law." Peacock's *Life*, p. 45. — *Translator.*

example serve as a lesson to so many young men whom political ambition diverts from a more noble vocation, to transform themselves into mere officials: but who might learn, like Young, to turn their eyes to the future, and not sacrifice to the futile and transitory satisfaction of being surrounded by persons soliciting favours, the solid testimonies of esteem and gratitude which the public rarely fails to offer to intellectual labours of a high order; and if it happen in the illusions of inexperience, that they should think too heavy a sacrifice imposed on them, we would ask them to take a lesson of ambition from the mouth of a great captain whose ambition knew no bounds; to meditate on the words which the First Consul, the victor of Marengo, addressed to one of our most honoured colleagues (M. Lemercier) on the day when he, quite in accordance with his character, had just refused a place then of great importance, that of Councillor of State: —

"I understand, Sir, you love literature, and you wish to belong altogether to it. I have nothing to oppose to this resolution. Yes! I, myself, if I had not become a General-in-chief, and the instrument of the fate of a great nation, do you think I would have gone through the offices and the salons, to put myself in dependence on whoever might happen to be in power in the position of minister or ambassador? No! no! I would have taken to the exact sciences. I would have made my way in the path of Galileo and Newton: and, since I have succeeded constantly in my great enterprises, truly I should have been equally distinguished by my scientific labours. I should have left behind me the remembrance of great discoveries. No other kind of glory would have tempted my ambition."

Young made choice of the profession of medicine, in which he hoped to find fortune and independence. His medical studies were commenced in London under Baillie and Cruikshank; he continued them at Edinburgh, where at that time Drs. Black, Munro, and Gregory were in the height of their celebrity. It was only at Göttingen, in the following year (1795), that he took the degree of Doctor.* Before going through this form, so empty, yet always so imperatively exacted, Young, hardly beyond the period of youth, had become known to the scientific world by a note relative to the gum ladanum; by the controversy which he sustained against Dr. Beddoes on the subject of Crawford's theory of heat; by a

* The author has omitted that, in 1797, Young entered as a fellow-commoner at Emmanuel College, Cambridge; and in due time graduated there regularly in medicine; a step at that time necessary for his admission to the College of Physicians, in order to enable him to practise as a physician in London. See Peacock's *Life*, p. 115. In the university he was familiarly known by the name of "Phenomenon Young."— *Translator.*

memoir on the habits of spiders, and the theory of Fabricius, the whole enriched with erudite researches; and lastly, by an enquiry on which I will enlarge on account of its great merit, the unusual favour with which it was received at its first production, and the neglect into which it has since fallen.

The Royal Society of London enjoys throughout the whole kingdom a vast and deserved consideration. The Philosophical Transactions which it publishes have been for more than a century and a half the glorious archives in which British genius holds it an honour to deposit its titles to the recognition of posterity. The wish to see his name inscribed in the list of fellow-labourers in this truly national collection, beside the names of Newton, Bradley, Priestley, and Cavendish has always been among the students of the celebrated universities of Cambridge, Oxford, Edinburgh, and Dublin*, the most anxious as well as legitimate object of emulation. Here is always the highest point of ambition of the man of science; he does not aspire to it unless on occasion of some capital investigation; and the first attempts of his youth come before the public by a channel better suited to their importance, by the aid of one of those numerous periodicals which, among our neighbours, have contributed so much to the progress of human knowledge. Such is the ordinary course; such consequently ought *not* to have been the course followed by Young; at the age of twenty he addressed a paper to the Royal Society. The council, composed of the most eminent men of the Society, honoured this paper with their suffrage, and it soon after appeared in the Philosophical Transactions. The author treated in it of the subject of vision.

THEORY OF VISION.

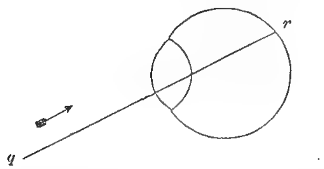
The problem was anything but new. Plato and his disciples, four centuries before our æra, were occupied with it: but at the present day their conceptions can hardly be cited but to justify the celebrated and little flattering sentence of Cicero: "There is nothing so absurd that it has not been said by some of the philosophers."

After passing over an interval of 2000 years, we must from Greece transport ourselves to Italy, if we would find any ideas on the wonderful subject of vision which merit the remembrance of the historian. Where, without having ever, like the philosopher of Egina, proudly closed their school against all who were not geometers, careful experimenters marked out the sole route by

* And, it might be added, probably to a far more numerous class not of those bodies.—*Translator.*

which it is permitted to man to arrive without false steps at the conquest of unknown regions of truth: there Maurolycus and Porta proclaimed to their cotemporaries that the problem of discovering *what is* presents sufficient difficulties to render it at least somewhat presumptuous to cast ourselves upon *the world of intelligences* to search after *what ought to be:* there these two celebrated fellow countrymen of Archimedes commenced the explanation of the functions of the different media of which the eye is composed: and showed themselves contented, as were at a later period Galileo and Newton, not to ascend above those kinds of knowledge which are capable of being elaborated or corrected by the aid of our senses, and which had been stigmatised under the porticoes of the Academy by the contemptuous epithet of *simple opinion.* Such is always human weakness that, after having followed with a rare success, the principal deviations which light undergoes in passing through the cornea and the crystalline, Maurolycus and Porta, when very near attaining their object, stopped short, as if before an insurmountable difficulty, when it was objected to their theory that objects ought to appear in an inverted position if the images formed in the eye are themselves inverted. The adventurous spirit of Kepler, on the contrary, did not remain embarrassed. It was from psychology that the attack originated; it was equally from psychology—clear, precise, and mathematical—that he overthrew the objection. Under the powerful hand of this great man, the eye became, definitively, the simple optical apparatus known by the name of the *camera obscura:* the retina is the ground of the picture, the crystalline replaces the glass lens.*

* The author seems to have left this illustration incomplete. Kepler's suggestion of the identity of the eye with the camera obscura, after all, does not touch the difficulty of the *inversion* of the image. Nor has it been considered as completely cleared up even till much later times. The solution which, it is believed, is now most generally assented to is this. It is a law of our constitution, dependent on some physiological principle unknown, that we refer impressions on the retina to objects existing, or believed to exist, in the rectilinear direction *from which* the impression comes to the retina. Consequently, as rays cross at the pupil, an impression arriving at (r) in the direction of the arrow,



will convey the idea of an object existing at (q); in other words, a ray falling on the *upper* part of the retina suggests an object lying *below*, or an *inverted* image suggests an *erect* object.

This assimilation, generally adopted since Kepler's time, remains open only to one difficulty: the *camera obscura,* like an ordinary telescope, requires to be brought to *a proper focus* according to the distance of objects. When objects are near it is indispensable to increase the distance of the picture from the lens: a contrary movement becomes necessary as they become more distant. To preserve to the images all the distinctness which is desirable, without changing the position of the surface which receives them, is therefore impossible: at least, always supposing the curvature of the lens to remain invariable; that it cannot increase when we look at near objects, or diminish for distant objects.

Among the different modes of obtaining distinct images, nature has assuredly made a choice, since man *can* see with great distinctness at *very different* distances. The question thus put has afforded a wide subject of remark and discussion to physicists, and great names have figured in the debate.

Kepler and Descartes held that the whole ball of the eye is susceptible of being elongated and flattened.

Porterfield and Zinn contended that the crystalline lens was moveable; and that it could place itself nearer to, or further from the retina, as might be needed.

Jurin and Musschenbroeck believed in a change in the curvature of the cornea.

Sauvages and Bourdelot supposed also that a change in curvature took place, but only in the crystalline lens. Such is also the system of Young. Two memoirs which our colleague successively submitted to the Royal Society of London include the complete development of his views.

In the first of these, the question is treated almost entirely in an anatomical point of view. Young there demonstrates by the aid of direct observations of a very delicate kind, that the crystalline is endowed with a fibrous or muscular constitution, admirably adapted to all sorts of changes of form. This discovery overthrew the only solid objection which had, till then, opposed the hypothesis of Sauvages and Bourdelot.

That hypothesis had no sooner been announced than it had been attacked by Hunter.

Thus this celebrated anatomist aided the cause of the young experimenter by the attention drawn to the subject, while his labours were as yet unpublished, and not even communicated to any one. However, this point of the discussion soon lost its importance. The learned Leuwenhoeck, armed with his powerful microscopes, traced out, and gave figures of, the muscular fibres in all their ramifications in the crystalline of a fish. To awaken the attention of the scientific world, tired with these long debates,

nothing less was necessary than the high renown of two new members of the Royal Society who entered the lists: one, a celebrated anatomist, the other the most eminent instrument maker of whom England could boast. These jointly presented to the Royal Society a memoir, the fruit of their combined labours, intended to establish the complete unalterability of the form of the crystalline. The scientific world was not prepared to admit that Sir Everard Home and Ramsden together, could possibly make inaccurate experiments, or be deceived in micrometical measurements. Young himself could not believe it: and in consequence he did not hesitate publicly to renounce his theory.

This readiness to own himself vanquished, so rare in a young man of twenty-five, and especially on the occasion of a first publication, was in this instance an act of modesty without example. Young, however, had really nothing to retract. In 1800, after having withdrawn his former disavowal, our colleague developed anew the theory of the change of form of the crystalline in a memoir against which, from that time, no serious objection has been brought.

Nothing could be more simple than his line of argument: nothing more ingenious than his experiments. Young, in the first instance, got rid of the hypothesis of a change of curvature *in the cornea* by the aid of microscopic observations, which were of a kind to render the most minute variations appreciable. We can say more: he placed the eye in special conditions where changes of curvature in the cornea would have been without effect: he plunged the eye in water, and proved that there was still the same faculty of seeing at different distances perfectly preserved. The second of three possible suppositions, that of an alteration in the dimensions of the whole organ, was again overthrown by a multitude of objections and of experiments which it was difficult to resist.

The problem thus seemed finally settled. Who does not see, in fact, that if, of three only possible solutions, two are put out of the question, the third is necessarily established: that if the radius of curvature of the *cornea* and the longitudinal *diameter of the whole eye* are invariable, it must follow that the form of the crystalline is invariable? Young, however, did not stop there; he proved directly, by the minute phenomena of the changes in the images, that the crystalline really changes its curvature; he invented, or at least, gave perfection to, an instrument susceptible of being employed even by the least intelligent persons, and those least accustomed to delicate experiments; and, armed with this new means of investigation, he assured himself that those individuals in whose eyes the crystalline has been removed in the operation

for cataract, did not enjoy the faculty of seeing equally distinctly *at all distances.**

We might fairly be astonished that this admirable theory of vision, this combination so well framed when the most ingenious reasonings and experiments lent each other mutual support, did not occupy that distinguished rank in the science of the country which it deserved. But to explain this anomaly, must we necessarily recur to a sort of fatality? Was Young then really, as he sometimes described himself with vexation, a new Cassandra, proclaiming incessantly important truths which his ungrateful contemporaries refused to receive? We should be less poetical, but more true, it seems to me, if we remarked that the discoveries of Young were not known to the majority of those who would have been able to appreciate them. The physiologists did not read his able memoir, because in it he presumes upon more mathematical knowledge than is usually attained in that branch.

The physicists neglected it in their turn, because in oral lectures, or printed works, the public demands little more at the present day than superficial notions, which an ordinary mind can penetrate without difficulty. In all this, whatever our distinguished colleague may have believed, we perceive nothing out of the ordinary course. Like all those who sound the greatest depths of science, he was misunderstood by the multitude; but the applauses of some of the select few ought to have recompensed him. In such a question we ought not to *count* the suffrages;—it is more wise to *weigh* them.†

* This instrument, called an "Optometer," was originally proposed by Dr. Porterfield, and consists of a simple and ingenious contrivance for ascertaining the focal length of the eye, which varies so greatly in different individuals, and often in two eyes of the same person, and in the same eye under different conditions. Dr. Young greatly improved upon the original construction. It will be found described in the *Lectures on Natural Philosophy*, vol. ii. p. 576. The principle of it consists in measuring accurately the distance of an object from the eye at which perfectly distinct vision is obtained, and which is determined when the object, seen through *two* small apertures close to the eye, presents only a single image, while in other positions it shows two images.—*Translator.*

† Arago, in assigning the probable causes of the neglect of Young's speculations, seems to fall short of his usual point and perspicuity. It might be true that his memoir was neglected by physiologists because it was mathematical, and by parity of reason it might have been neglected by physicists and mathematicians as being physiological. But it is surely no reason to say that it was neglected by physicists *because* the public are superficial, &c. Young may have been in most of his speculations too profound for the many; but this particular instance of the structure of the eye and theory of vision is, perhaps, of all his researches, that which can be the least open to this charge. The subject is not itself abstruse: it is one easily understood by every educated person, without mathematical attainments; and the point at issue was a simple question of fact requiring no profound physiological knowledge to appreciate, whether

INTERFERENCES.

The most beautiful discovery of Young, that which will render his name imperishable, was suggested to him by an object in appearance very trivial; by those soap bubbles so brilliantly coloured, so light, which when just blown out of a pipe become the sport of every imperceptible current of air. Before so enlightened an audience, it would without doubt be superfluous to remark that the difficulty of producing a phenomenon, its variety, its utility to the arts, are not the necessary indications of its importance in a scientific point of view. I have, therefore, to connect with a child's sport the discovery which I proceed to analyse, with the certainty that its credit will not suffer from its origin. At any rate I shall have no need to recall the apple, which, dropping from its stalk and falling unexpectedly at the feet of Newton, developed the ideas of that great man respecting the simple and comprehensive laws which regulate the celestial motions; nor the frog and the touch of the bistoury, to which physical science has recently been indebted for the marvellous pile of Volta. Without referring in particular to soap bubbles, I will suppose that a physicist has taken for the subject of experiment some distilled water, that is to say, a liquid, which in its state of purity never shows any more than some very slight shade of

the crystalline has or has not a muscular structure capable of changing its convexity. The real state of the case seems to be very satisfactorily explained by Dean Peacock (p. 36. *et seq.*), from whose account, as well as from what has been since written, it appears, after all that has been done both by Dr. Young and others, that there is even at the present day considerable difference of opinion on the subject.

Perhaps the most comprehensive survey of the whole subject which recent investigation has produced will be found in the paper of Professor J. D. Forbes in the *Edin. Transactions*, vol. xvi. pt. I. 1845. After giving a summary view of preceding researches, and adverting to the prevalent opinion among men of science, that the true explanation yet remains to be discovered (most anatomists denying as a fact the existence of the *muscular* structure which Young conceived he had proved), Professor Forbes proposes, as his own view of the cause, the consideration of the remarkable *variation in density* of the crystalline towards its central part; coats of different density being disposed in different layers, may be acted on by the pressure of the humours of the eye when the external action of the muscles compresses them, and thus increase the curvature of the lens, when the eye is directed to a near object, the whole consistence especially in the outer parts being of a gelatinous or compressible nature, and the central part more solid and more convex. Thus uniform pressure on the outer parts would tend to make the outer parts conform more nearly to the more convex interior nucleus.

It may be added that many physiologists are of opinion that, after all, there does not exist a sufficient compressive action on the ball of the eye to produce the effect supposed. — *Translator.*

colour, blue or green hardly sensible, and that only when the light traverses great thicknesses. I would next ask what we should think of his veracity if he were to announce to us, without further explanation, that to this water, so limpid, he could at pleasure communicate the most resplendent colours; that he knew how to make it violet, blue, green; then yellow like the peel of citron, or red of a scarlet tint, without affecting its purity, without mixing with it any foreign substance, without changing the proportions of its constituent gaseous elements. Would not the public regard our physicist as unworthy of all belief, especially when, after such strange assertions, he should add, that to produce colour in water, it suffices to reduce it to the state of a thin film; that "thin" is, so to speak, the synonym of "coloured;" that the passage of each tint into one the most different from it is the necessary consequence of a simple variation of the thickness of the liquid film; that this variation, for instance, in passing from red to green, is not the thousandth part of the thickness of a hair! Yet these incredible propositions are only the necessary consequences deduced from the accidental observation of the colours presented by soap bubbles, and even by extremely thin films of all sorts of substances.

To comprehend how such phenomena have, during more than 2000 years, daily met the eyes of philosophers without exciting their attention, we have need to recollect to how few persons nature imparts the valuable faculty of being astonished to any purpose.

Boyle was the first to penetrate into this rich mine. He confined himself, however, to the minute description of the varied circumstances which gave rise to these iridescent colours. Hooke, his fellow-labourer, went further. He believed that he had discovered the cause of this kind of colours in the coincidences of the rays, or to speak in his own language, in the mutual action on each other of the *waves* reflected by the two surfaces of the thin film. This was, we may admit, a suggestion characteristic of genius; but it could not be made use of at an epoch when the compound nature of white light was not as yet understood.

Newton made the colours of thin films a favourite object of study. He devoted to them an entire book of his celebrated treatise the "Optics." He established the laws of their formation by an admirably connected chain of experiments, which no one has since surpassed in excellence. In illuminating with homogeneous light the very regularly formed series of bands of which Hooke had already made mention, and which originated round the point of contact of two lenses pressed closely together, he proved that for each species of simple colour there exists, in

thin films of every substance, a series of thicknesses gradually increasing, at each of which no light is reflected from the film. This result was of capital importance; it included the key to all these phenomena.

Newton was less happy in the theoretical views which these remarkable observations suggested to him. To say, with him, that the luminous ray which is reflected is "in a fit of easy reflexion,"— to say that the ray which passes through the film entire, is "in a fit of easy transmission,"— what is it but to announce, in obscure terms, merely the same fact which the experiment with the two lenses has already taught us? *

The theory of Thomas Young is not amenable to this criticism. Here there is no longer admitted any peculiar kind of "fits" as primordial properties of the rays. The thin film is here assimilated in all respects to any thicker reflector of the same substance. If at certain points in its surface no light is visible, Young did not conclude that therefore its reflexion had ceased: he supposed that, in the special directions of those points, the rays reflected by the second surface proceeded to meet with those reflected from the first surface, and completely destroyed them. This conflict of the rays is what the author designated by the term "*interference,*" which has since become so famous.

Observe then here the most singular of hypotheses! We must certainly feel surprised at finding night in full sunshine, at points where the rays of that luminary arrive freely; but who would

* In regard to the theory of the "fits," the author here seems to represent Newton's view, as in fact mere tautology; while in other places he is supposed to have indulged in a visionary theory on the subject. Newton, however, expressly says, "what kind of action or disposition this is; — whether it consist in a circulating or vibrating motion of the ray, or of the medium, or something else, I do not here inquire." (*Optics,* p. 255. ed. 1721.)

The fact is, Newton in his optical researches expressed the same avowed and systematic dislike to indulging in *any* gratuitous theories as in his other inquiries. "Hypotheses non fingo," was his motto in these as well as other researches. In adopting the idea of "fits of easy reflexion and transmission," we are of opinion that he did not violate that maxim, and that it was in fact the only legitimate first expression of the conclusion which the facts warranted. At certain points *no light appeared;* it was the legitimate inference, in the then state of knowledge, *that none was reflected.* But light was clearly under the same cicumstances *transmitted;* at a distance a little greater along the ray, an opposite effect was witnessed; and so on. It was nothing more than the strict inference that at those points successively *something occurred* in the course of the ray which disposed it for, or induced, reflexion in the one case, and non-reflexion in the other; accompanied in the latter case by the like tendency to transmission. These apparent "fits" must be still acknowledged as *phenomena;* the *mechanism* by which they are produced is, however, now known to be nothing inherent in the light, no essential property recurring, but the simple periodicity of conspiring or counteracting wave action. — *Translator.*

have imagined that we should thence come to suppose th: darkness could be engendered by adding light to light!

A physicist is truly eminent when he is able to announce an result which, to such an extent, clashes with all received ideas but he ought, without delay, to support his views by demor strative proofs, under the penalty of being assimilated to thos Oriental writers whose fantastic reveries charmed the thousan and one nights of the Sultan Schahriar.

Young had not this degree of prudence. He showed at onc that his theory would agree with the phenomena, but withou going beyond mere possibility. When at a later period he arrive at real proofs of it, the public had other prepossessions, which h was not able to overcome. However, the experiment, whenc our colleague deduced so memorable a discovery, could not excit the shadow of a doubt.*

* In the retrospective glance which the author thus gives over the progress o discovery previous to the period at which Dr. Young first entered on the field what we have chiefly to observe is, that up to that date nothing like a *connecte view* of the physical character of this wonderful agent had been attained; : few isolated speculations had indeed been put forth respecting a theory o emitted molecules on the one hand, and of waves in an æthereal medium on th other; and a few experimental facts bearing on the choice between sucl hypotheses had been ascertained.

The several distinct phenomena of common reflexion and refraction, of doubl refraction, of inflexion or diffraction, and of the coloured rings, did not seem t be connected by any *common* principle; nor, even separately considered, could i be said that they were very satisfactorily explained. It was now the peculiar distinction of Young to perceive, and to establish in the most incontestible manner, a great principle of the simplest kind, which at once rendered the wave hypothesis applicable to the two last named classes of facts, and thus directly connected them with the former.

It is not always that we are enabled to trace the first rise and progress of the idea of a great discovery in the inventor's mind. We cannot forbear from here noticing, that Dr. Young has left on record the progress of the first suggestions which occurred to him on the subject of interference. The first view which presented itself was that of the *analogies* furnished by *sound*, which, as is well known, is conveyed by means of waves propagated in air. And in the case of two sounds differing a very little from the same pitch, produced at the same time, we have, not a continuous sound, but *beats*, that is, alternations of sound and silence; the waves in the one case conspiring with and reinforcing each other, in the other counteracting, neutralising, and destroying each other.

But in more special reference to light, Dr. Young's account of the origin of his ideas is so clear and striking that we must give it in his own words:— "It was in May, 1801, that I discovered, by reflecting on the beautiful experiments of Newton, a law which appears to me to account for a greater variety of interesting phenomena than any other optical principle that has yet been made known. I shall endeavour to explain this law by a comparison:— Suppose a number of equal waves of water to move upon the surface of a stagnant lake, with a certain constant velocity, and to enter a narrow channel leading out of the lake;— suppose, then, another similar cause to have excited

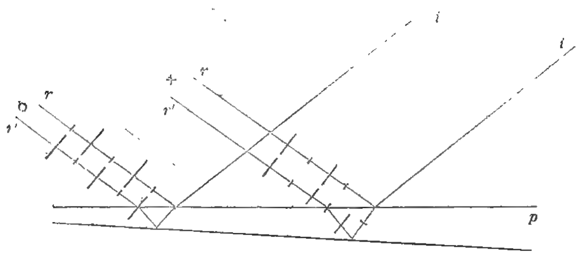
Two rays, proceeding from the same source by slightly unequal routes, crossed one another at a certain point in space. At this

another equal series of waves, which arrive at the same channel with the same velocity, and at the same time with the first. Neither series of waves will destroy the other, but their effects will be combined; if they enter the channel in such a manner that the elevations of the one series coincide with those of the other, they must together produce a series of greater joint elevations; but if the elevations of one series are so situated as to correspond to the depressions of the other, they must exactly fill up those depressions, and the surface of the water must remain smooth; at least, I can discover no alternative, either from theory or from experiment. Now, I maintain that similar effects take place whenever two portions of light are thus mixed; and this I call the general law of the interference of light."[1] — *Translator.*

For the sake of many readers, it may not be superfluous or useless here briefly to illustrate the application of these theoretical ideas. We have only to imagine in like manner, in the case of the rays of light, two sets of waves propagated through an æthereal medium and coinciding in direction, when it will be easily apparent that just as in the case of the supposed canal, they may have their waves either conspiring or counteracting, and consequently giving a point of brightness or darkness accordingly.

Thus, a coincidence in the periods, or an interval of an integer number of entire wave-lengths, would cause the two systems of waves to conspire and reinforce each other; a difference of periods of half a wave-length, or any odd number of half wave-lengths, would cause the two systems to counteract or neutralise each other. Thus, according to the thickness, there would be a point of darkness or of brightness for each primary ray, and the succession of tints would be perfectly explained.

This would directly apply to the *thin films.* A ray impinging would be partly reflected at the first surface of the thin film, partly entering it would be reflected internally at its second surface, and emerge coinciding in *direction* with the first, but retarded behind it from the thickness traversed in its *undulations* either by a whole, or half undulation, or some multiples of these,— thus giving either a point of brightness or one of darkness accordingly; or by some intermediate fraction, giving an intermediate shade. And this would go on alternately at successively greater thicknesses of the film, giving a succession of such points or bands.



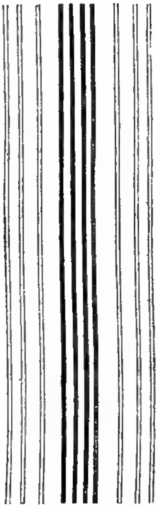
Thus at two successive thicknesses of the plate (p), the incident rays falling on it in parallel directions, i i, are reflected partially from the first surface, r r, and partially from the second, r' r'. According to the difference of thickness

[1] Works, vol. i. p. 202.

point was placed a sheet of white paper. Each ray, taken by itself, made the paper more bright at that point, but when the two

traversed, these may be in accordance giving a point of brightness as at +, or in discordance giving a point of darkness as at o.

If two rays or sets of waves, instead of being exactly super-imposed be supposed to *meet* inclined at a very acute angle, in a somewhat similar way they would, at a series of points, alternately conspire or clash with each other, thus giving rise to a series of bright and dark points, the assemblage of which will produce bands or stripes on a screen intercepting the rays. Now as to actual experimental cases, it was in the application of this latter theoretical idea that the invention of Dr. Young was peculiarly displayed. The former case was that alone which seems to have occurred to Hooke in reference to the colours of thin plates, and even this was in his mind but a very indefinite conception; nor did it seem at first sight readily comparable with such cases as the diffraction fringes, or still less with the internal bands of a shadow observed by Grimaldi. If Hooke had imagined any theoretical views of this kind, it was probably confined to the one case of the thin films: Young's great merit was the comprehensiveness of his principle; and in following out the investigation, he proceeded at once to such a generalisation as evinced that comprehensiveness and connected immediately those classes of phenomena apparently so different in character,—the thin films, the internal bands, and the external fringes. When, as in Grimaldi's



experiment (since called the phenomena of diffraction), a narrow slip of card was placed in a very narrow beam of solar light, dark and bright stripes parallel to the sides internally marked the whole shadow longitudinally, while the external fringes appeared on the outside at each edge. The general appearance of the shadow of a long narrow body with parallel sides in a beam of solar light issuing from a minute hole in a shutter, or, what is better, the focus of a small lens collecting the rays to a point, is that of a shadow marked with longitudinal stripes and externally bordered by parallel fringes or bands of light slightly coloured, as seen in the annexed figure.

To *exhibit these* appearances ordinarily requires the sun's light. But the translator has found a very simple method of exhibiting these phenomena on a

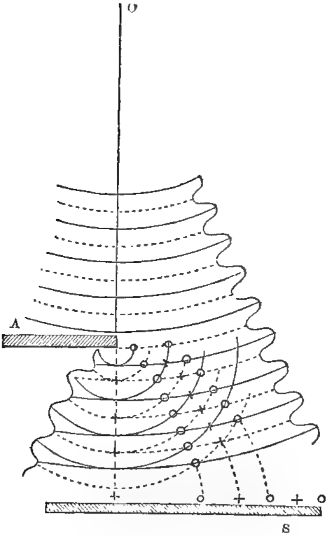
rays united and arrived at that point together, all brightness disappeared; complete night succeeded to day.

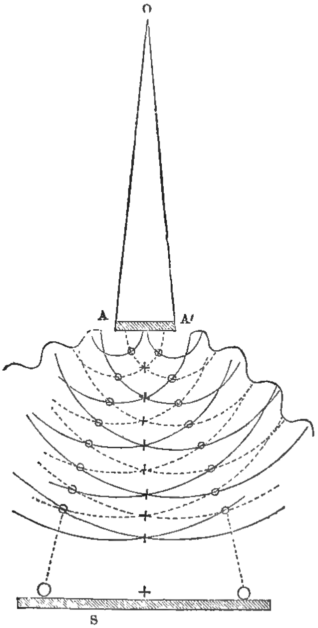
Two rays do not always annihilate each other completely at their point of intersection. Sometimes we observe only a partial weakening of intensity, sometimes, on the other hand, the rays conspire and increase the illumination. Everything depends on

minute scale by candle light, by merely placing a fine wire across one surface of a lens of short focus, and looking through it at light admitted through a narrow slit parallel to the wire, or even the flame of a candle at a considerable distance.

Next, as to the theoretical explanation, an inspection of the accompanying diagram will perhaps help to convey an idea of the manner in which the several sets of waves are formed and interfere in the case now supposed.

Young conceived the beam of light as a series of waves propagated onward, till, on reaching the card, they were broken up into two new sets of waves spreading in circles round each edge as a new centre, while part of the original set continued to pass on at each side. On the principle just mentioned these would *interfere* with the new portions on the outside; and the two new portions would *interfere* with each other in the inside of the shadow; in either case giving stripes or bands. To complete the proof, when an opaque screen was





placed so as to intercept the rays on one side, though abundance of light was present on the other, yet all the internal bands immediately disappeared; demonstrating that the effect was due solely to the *concurrence* of the light from *both* sides. The bands produced by light admitted through narrow apertures, and numerous other phenomena of the same kind, may receive a general and popular explanation in the same way.— *Translator*.

the difference in the length of route which they have gone through, and that according to very simple laws, the discovery of which in any age would suffice to immortalise a physicist.

The differences of route which produce these conflicts between the rays, accompanied by their entire mutual destruction, have not the same numerical value for the differently coloured primary rays. When two white rays cross, it is then possible that one of their chief constituent parts, the red for example, may alone be in the condition fit for mutual destruction. But white, deprived of its red, becomes green! Thus interference of light manifests itself in the phenomena of coloration. Thus the different elementary colours are placed in evidence without any prism to separate them. We should, however, remark that there does not exist a single point in space where a thousand rays of the same origin do not proceed to cross one another after reflexions more or less oblique, and we shall perceive at a glance the whole extent of the unexplored region which interferences open to the investigations of experimenters.

When Young published this theory, many phenomena of periodical colours had been already offered to the notice of observers; and we should add, had resisted all attempts at explanation. Among the number we might instance the coloured rings which are formed by reflexion, not on thin films, but on mirrors of thick glass slightly concave; the iridescent bands of different breadths with which the shadows of bodies are bordered on the outside, and in some instances covered within, which Grimaldi first noticed, and which afterwards uselessly exercised the genius of Newton, and of which the completion of the theory was reserved for Fresnel; the bows coloured red and green, which are perceived in greater or less number immediately under the innermost of the prismatic bands of the rainbow*, and which seemed so completely inexplicable, that the writers of elementary books on physics had given up making mention of them; and lastly, the "coronas," or broad coloured circles with varying diameters, which often appear surrounding the sun and moon.

If I call to mind how many persons do not appreciate scientific theories, except in proportion to the immediate applications which they may offer, I cannot terminate this enumeration of the phenomena which characterise the several series of more or less numerous periodical colours, without mentioning the rings, so remarkable by their regularity of form and purity of tint with which every brilliant light appears surrounded, when we look at

* This explanation has been recently controverted by Professor Potter.—*Philos. Mag.* May, 1855.

it through a mass of fine molecules or filaments of equal dimensions. These rings, in fact, suggested to Young the idea of an instrument, extremely simple, which he called an "eriometer," and with which we can measure without difficulty the dimensions of the most minute bodies. The eriometer, as yet so little known to observers, has an immense advantage over the microscope in giving at a single glance the *mean magnitude* of millions of particles which are contained in the field of view. It possesses, moreover, the singular property of remaining *silent* when the particles differ much in magnitude among themselves, or, in other words, when the question of determining their dimensions has no real meaning.

Young applied his eriometer to the measurement of the globules of blood in different classes of animals, — to that of powders furnished by different species of vegetables, — of the fineness of different kinds of fur used in the manufacture of different fabrics, from that of the beaver, the most valuable of all, down to that of the common sheep of the Sussex breed, which stands at the other extremity of the scale, and is composed of filaments four times and a half thicker than that of the beaver.

Before the researches of Young, the numerous phenomena of colours* which I have just pointed out were not only inexplicable, but nothing had been found to connect them with each other. Newton, who was long engaged on the subject, had not perceived any connexion between the rings in thin films and the bands of diffraction. Young reduced these two kinds of coloured bands alike to the law of interference. At a later period, when the coloured phenomena of polarization had been discovered, he observed, in certain measures of the thicknesses at which they occurred, some remarkable numerical analogies, which made it very reasonable to expect that sooner or later this singular kind of polarization would be found connected with his doctrine. He had in this instance, however, we must admit, a very wide hiatus to fill up. The knowledge of some important properties of light, then completely unknown, would have been necessary to permit him to conceive the whole singularity of the effects which in certain crystals, cut in certain directions, double refraction produces by the destruction of light resulting from the interference of rays;

* Every one may have remarked the threads of a spider's web occasionally exhibiting brilliant colours in the sunshine. The same thing is seen in fine scratches on the surface of polished metal, produced in a more regular way, by the fine engraved parallel grooves in Barton's buttons. The colours of mother-of-pearl are of the same kind; all these colours Dr. Young showed were due to *interference* of the portions of light reflected from the sides of the narrow transparent thread or groove. — *Translator.*

but it is to Young that the honour belongs of having opened the way; it was he who was the first to decypher these hieroglyphics of optics.*

* It has been well observed that simplicity is not always a fruit of the first growth, and accordingly some of the earliest of Young's researches were complicated by unnecessary conditions. Thus, to exhibit the effect of *two* rays interfering, he at first not unnaturally transmitted the narrow beam of light through *two* small apertures near together. In point of fact, though the real effect is here seen, it is mixed up with others of a more complex kind. The narrow apertures each exhibited coloured fringes, in addition to the interference stripes seen between them. The coloured fringes of *apertures* (unless very wide) are distinct from those formed by one external edge of an opaque body; the light from *each side* conspires to the effect in a somewhat complex manner. If the aperture be otherwise than long with parallel sides, the phenomenon becomes still more complex, and the calculation difficult; few such cases have ever yet been solved, and some such cases have been dwelt upon as formidable objections to the theory; they are simply cases to which the formula, from its mathematical difficulties, has not *yet* been extended.

In all these cases of diffraction an *opaque* body was used, and it might still be suspected that *some action of the edge* of that body might be concerned in the result. Numerous experiments of Maraldi, Dutour, Biot, and others, were directed to the investigation of this point. Biot showed that an *opaque* body was not necessary, inasmuch as the edge of a plate *of glass*, or even the bounding line of two faces of a glass cut at a slight inclination to each other, gave the same fringes; indeed, Newton also had noticed something of the kind. Haldat varied the conditions of the edge in every conceivable way, whether of form or nature, by the influence of magnetism, galvanism, electricity, or temperature from freezing to a red heat, without producing the slightest difference in the fringes; a result which it would be impossible to conceive compatible with any idea of an atmosphere of attraction or repulsion surrounding the edge.

Again, though we have given the explanation of the *external* fringes in its simple and correct form, yet both Young and Fresnel failed in the first instance to see it in that light, both believing that the *reflexion* of a portion of rays from the *edge* of the opaque body was *mainly* concerned in producing the interference. Subsequent experiments showed that even in cases where that edge reflects any sensible amount of light, its influence on the diffracted fringes is quite inappreciable. In fact, Young, in a letter to Fresnel in returning thanks for a copy of a later memoir in which he had shown this supposition to be unnecessary, also concurs in abandoning it. It did but complicate and injure the beauty of the result.[1] And every doubt must have disappeared in the minds of those who compared the minute arithmetical accuracy with which the places of the fringes, as computed from the simple theory in the investigations of Fresnel, agreed with those actually determined by the nicest micrometrical measurements.

In enumerating the discoveries of Young in the first establishment of the wave theory, it is somewhat singular that Arago (whether from accident or design) should have overlooked one investigation which must be regarded as among the most important. The great support which the emission theory received in recent times was that derived from La Place's memoir on the law of double refraction (1809), in which, on the principle of "least action," as main-

[1] Young's Works, i. 393.

EGYPTIAN HIEROGLYPHICS.—HISTORY OF THE FIRST EXACT INTERPRETATION GIVEN OF THEM.

The word hieroglyphic, regarded not metaphysically, but in its natural acceptation, carries us into a field which has been long the theatre of numerous and animated debates. I have hesitated whether to risk offending the feelings which this question has excited. The secretary of an Academy occupied exclusively with the exact sciences, might indeed, without impropriety, remit this philological subject to other more competent judges. I also feared, I will avow, to find myself in disagreement on several important points with the illustrious man of science whose labours it has been so delightful to me to analyse, without having to add a word of criticism from my pen. All these scruples, however, vanish when I reflect that the interpretation of hieroglyphics has been one of the most beautiful discoveries of our age; that Young himself has mixed up my name with discussions relating to it; that to examine whether France can pretend to this new title to glory, is to enhance the importance of the task confided to me at this moment, and to perform the duty of a good citizen. I am aware that some

tained by Maupertuis and applied to the idea of luminous molecules, he explained the observed laws of ordinary and extraordinary refraction in Iceland spar. This investigation exercised a powerful influence in favour of the molecular theory over the minds of the men of science in France who bowed implicitly to the authority of Laplace. But the memoir of Laplace was the subject of a very powerful attack on the part of Dr. Young, carried on in an article in the Quarterly Review, November, 1809, in which he disputed the mechanical and mathematical grounds of Laplace's theory, and showed that the same laws of double refraction could be far more easily deduced from the undulatory hypothesis. Next to the discovery of interference, this refutation of the strongest point of the emission theory cannot but be regarded as one of the most material in the development and establishment of the undulatory view.

To the statement of these various cases of interference it should be added that when the tints of polarized light were discovered, Young in 1814 applied to the phenomena the *general* consideration of *interference*, that is to say, he showed that owing to the differing obliquities of the paths of the rays within the crystal they would be unequally retarded in their passage, and would consequently emerge in conditions, with regard to length of route, respectively of accordance or discordance at corresponding distances round the central line or axis of the crystal, and thus might give rise to coloured rings. Arago, however, soon noticed that the explanation was incomplete; the main point in fact remained to be accounted for, viz. why we see no colours till the *analyser* is applied, and why even the previous polarization is necessary to the result. It was not until about two years afterwards that Arago and Fresnel jointly succeeded in discovering a new law, which not only furnished the complete solution of the polarized rings, but at length cleared away all the difficulties which from the first had surrounded the idea of polarization itself. For an account of this see memoir of Fresnel.—*Translator.*

may find narrowness in these sentiments. I am not ignorant that the cosmopolitan spirit has its good side; but with what name shall I stigmatise it, if, when all neighbouring nations enumerate with triumph the discoveries of their sons, it should hinder me from seeking, even in the present circle, among those colleagues whose modesty I would not hurt, the proof that France is not degenerate; that she also adds every year her glorious contingent to the vast deposit of human knowledge.*

I approach, then, the question of Egyptian writing, and I do so free from all prejudice, with the firm wish of being just; with the lively desire to conciliate the rival pretensions of two men of science whose premature death has been to all Europe a legitimate subject of regret. Lastly, I shall not in this discussion on hieroglyphics transgress the bounds imposed on me; happy if those who listen to me, and whose indulgence I ask, may find that I have known how to escape the influence of a subject whose obscurity is proverbial.

Men have imagined two systems of writing entirely distinct. One is that employed by the Chinese, which is the system of hieroglyphics, the other, at present in use among all other nations, bears the name of the alphabetical or phonetic system.

The Chinese have no letters properly so called: the characters which they use in writing are strictly hieroglyphics; they do not represent *sounds* or articulations, but *ideas.* Thus a house is represented by a unique and special character, which does not change even when the Chinese have come to call a house, in their spoken language, by a name totally different from that which they formerly pronounced. Does this result appear surprising? Imagine the case of our cyphers, which are also hieroglyphics; the idea of one added to itself seven times is expressed everywhere in France, in England, in Spain, &c., by the aid of two circles placed vertically one over the other, and touching in one point; but in looking at this hieroglyphic sign (8) the Frenchman pronounces "huit," the Englishman "eight," the Spaniard "ocho." No one is ignorant that it is the same with compound numbers. Thus, to

* In bringing out a part of this chapter on Egyptian Hieroglyphics in the Annuaire for 1836, Arago has added:—"The first exact interpretation which has been given of Egyptian hieroglyphics will certainly take its place among the most beautiful discoveries of the age. Besides, after the animated debates to which it has given birth, every one would desire to know whether France can *conscientiously* pretend to this new title to glory. Thus the importance of the question, and the national self-love properly understood, unite in encouraging me to publish the result of a minute examination to which I have devoted myself. Can I, then, be blind to the danger which there always is in attempting difficult subjects in matters which we have not made the special subject of our studies?"

speak briefly, if the Chinese idiographic signs were generally adopted, as the Arabic numerals are, every one would read in his own language the works which they presented to him, without the need of knowing a single word of the language spoken by the authors who have written them.

It is not so with alphabetical writing:

> "He who first taught us the ingenious art
> To paint our words, and speak them to our eyes."

having made the capital remark that all words of a spoken language, even the most rich, are compounded of a very limited number of elementary articulate sounds, invented artificial *signs* or letters to the number of twenty-four or thirty to represent them. By the aid of these signs differently combined he could write every word which struck his ear even without knowing the meaning of it.

The Chinese or hieroglyphic writing seems to be the infancy of the art. It is not always, as has been sometimes said, that to learn to read it, even in China, occupies the whole life of a studious Mandarin. Rémusat (whose name I cannot mention without recalling one of the most heavy losses which literature has lately sustained) has established, both by his own experience and by the fact of the excellent scholars he has formed every year by his lectures, that we may learn Chinese like any other language. It is not true, as was once imagined, that the characters are appropriated solely to the expression of common ideas; several pages of the romance of Yu-kiao-li, or the Two Cousins, will suffice to show that the most subtle abstractions, the quintessence of refinements, are not beyond the range of the Chinese writing. The chief fault of this mode of writing is, that it gives no means of expressing new names. A letter from Canton might have told at Pekin, that on the 14th of June, 1800, a great and memorable battle saved France from great peril; but it would not have been able to express in these purely hieroglyphic characters that this glorious event took place near the village of Marengo, or that the victorious general was called Buonaparte. A people among whom the communication of proper names, from one place to another, could only take place by means of special messengers, would be, as we see, only in the first rudiments of civilisation. These preliminary remarks are not useless. The question of priority, which the graphic methods of Egypt have called forth, thus come to be easy to explain and to comprehend. As we proceed, in fact, we find in the hieroglyphics of the ancient people of the Pharaohs, all the artifices of which the Chinese make use at the present day.

Many passages of Herodotus, of Diodorus Siculus, of Clemen of Alexandria, have taught us that the Egyptians had two or thre different sorts of writing, and that in one of these, at least, sym bolic characters, or the representatives of ideas, played a principa part. Horapollon has even preserved to us the signification of a certain number of these characters. Thus we know that the *hawl* designated *the soul;* the *ibis,* the *heart;* the *dove* (which migh seem strange), a *violent man;* the *flute,* an *alien;* the number *six pleasure;* a *frog,* an *imprudent man;* the *ant, wisdom;* a *running knot, love;* &c.

The signs thus preserved by Horapollon form only a very smal part of the eight or nine hundred characters which have beer found in the ancient inscriptions. The moderns, Kircher among others, have endeavoured to enlarge the number. Their efforts have not given any useful result, unless it be so to show to wha errors even the best instructed men expose themselves when, ir the search after facts, they abandon themselves without restrain to imagination. In the want of data, the interpretation of th Egyptian writings appeared for a long time, to all sound minds, a problem completely incapable of solution; when in 1799, M Boussard, an engineer officer, discovered in the excavations whicl he was making near Rosetta, a large stone covered with in scriptions in three kinds of characters quite distinct.

One of the series of characters was Greek. This, in spite o some mutilations, made clearly known that the authors of th monument had ordained that the same inscription should be trace in three different sorts of characters, viz. in the sacred character or Egyptian hieroglyphics, in the local or vulgar characters, an in Greek. Thus, by an unexpected good fortune, the philologist found themselves in possession of a Greek text, having als before them its translation into the Egyptian language, or at leas a transcription in two sorts of characters, anciently in use on th banks of the Nile.

This Rosetta stone, since so celebrated, and which M. Boussar presented to the Institute of Cairo, was taken from that body a the period when the French army evacuated Egypt. It wa preserved, however, in the British Museum, where it figured, a Thomas Young said, as a monument of British valour. Puttin valour out of the question, the celebrated philosopher might hav added, without too much partiality, that this invaluable trilingua monument thus bears some witness to the advanced views whic guided all the details of the memorable expedition into Egypt, a also to the indefatigable zeal of the distinguished savants whos labours, often carried on under the fire of the forts, have added s much to the glory of their country. The importance of th

Rosetta stone struck them, in fact, so forcibly, that in order not to abandon this precious treasure to the adventurous chances of a sea voyage, they earnestly applied themselves, from the first, to reproduce it, by copies, by impressions taken in the way of printing from engravings, by moulds in plaster or sulphur. We must add that antiquaries of all countries became first acquainted with the Rosetta stone from the designs given by the French savants.

One of the most illustrious members of the Institute, M. Silvestre de Sacy, entered first in 1802 on the career which the trilingual inscription opened to the investigations of philologists. He only occupied himself on the Egyptian text in common characters. He there discovered the groups which represent the different proper names, and their phonetic nature. Thus in one of two inscriptions, at least, the Egyptians had the signs of sounds, or true letters. This important result found no opponents after a Swedish man of science, M. Akerblad, in completing the labours of our fellow-countryman, had assigned, with a probability bordering on certainty, the phonetic value of each of the different characters employed in the transcription of the proper names which the Greek text disclosed.

There remained, all along, the purely hieroglyphic part of the inscription, or what was supposed such; this remained untouched; no one had ventured to attempt to decypher it.

It is here that we find Young declaring, as if by a species of inspiration, that in the multitude of sculptured signs on the stone representing either entire animals, or fantastic forms, or again instruments, products of art, or geometrical forms, those of these signs which were found enclosed in elliptic borders, corresponded to the proper names in the Greek inscription; in particular to the name of Ptolemy, the only one which in the hieroglyphic inscription remains uninjured. Immediately afterwards Young said that in the special case of the border or scroll, the signs included represented no longer ideas, but sounds. In a word, he sought by a minute and refined analysis to assign an individual hieroglyphic to each of the sounds which the ear receives in the name of Ptolemy in the Rosetta stone, and in that of Berenice, in another monument.

Thus we see, unless I mistake, in the researches of Young on the graphic systems of the Egyptians, the three culminating points. No one, it is said, had perceived them, or at least had pointed them out, before the English philosopher. This opinion, although generally admitted, appears to me open to dispute. It is, in fact, certain that in 1766 M. de Guignes, in a printed memoir, had indicated that the *scrolls* in Egyptian inscriptions included all the

proper names. Every one might also see in the same work the arguments on which the learned orientalist relied to establish the opinion which he had embraced on the constant phonetic character of the Egyptian hieroglyphics. Young then has the priority on this point alone: to him belongs the first attempt which had been made to decompose in letters the groups of the scrolls, to give a phonetic value to the hieroglyphics which composed in the stone of Rosetta the name of Ptolemy.

In this research, as we might expect, Young furnished new proofs of his immense penetration; but misled by a false system, his efforts had not a full success. Thus sometimes he attributes to the hieroglyphic characters a value simply alphabetical, further on he gives them a value which is syllabic or disyllabic, without being struck by what must seem so strange in this mixture of different characters. The fragment of an alphabet published by Young includes then something both of truth and falsehood; but the false so much abounds that it would be impossible to apply the value of the letters which compose it to any other reading than that of the two proper names from which it was derived. The word *impossible* is so rarely met with in the scientific career of Young, that I must hasten to justify it. I will say then that after the composition of his alphabet Young himself believed that he saw in the scroll of an Egyptian monument the name of "*Arsinoe*," where his celebrated competitor had since shown with irresistible evidence the word "*autocrator:*" that he believed he had found "*euergetes*" in a group where we ought to read "*Cæsar*."

The labours of Champollion, as to the discovery of the phonetic value of hieroglyphics, are clear, distinct, and cannot involve any doubt. Each sign is equivalent to a single vowel or consonant. Its value is not arbitrary: every phonetic hieroglyphic is the image of a physical object whose name in the Egyptian language commences with the vowel or the consonant which it is wished to represent.*

* This will become clear to every one, if we seek, by following the Egyptian system, to compose hieroglyphics in the French language. A may be represented by (agneau) a lamb; (aigle) an eagle; an ass, anemone, artichoke, &c. B by a balance, a whale (baleine); a boat, &c. C by cabana (badger); cheval (horse); cat, cedar, &c. E by épée (a sword), elephant, epagneul (spaniel), &c. Abbé then would be written in French hieroglyphics by putting any of the following figures in succession: — a lamb, a balance, a whale, an elephant. Or an eagle, a boat, a sword, &c.

This kind of writing has some analogy, as we see, with the rebus in which confectioners wrap their bonbons. Thus we see at what stage these Egyptian priests were of whom antiquity has so much boasted, but who, we must say, have taught us so little.

M. Champollion calls *homophones* all those signs which, representing the same

The alphabet of Champollion, once modelled from the Stone of Rosetta and two or three other monuments, enables us to read inscriptions entirely different; for example, the name of Cleopatra on the obelisk of Philoë, long ago transported into England, and where Dr. Young, armed with his alphabet, could discover nothing. On the temple of Karnac, Champollion read twice the name of Alexander: on the Zodiac of Denderah, the title of a Roman emperor; on the grand edifice above which it is placed, the names and surnames of the emperors Augustus, Tiberius, Claudius, Nero, Domitian, &c. Thus, to speak briefly, we find, on one hand, the lively discussion, to which the age of these monuments had given rise, completely terminated; on the other, we observe it established beyond question that under the Roman dominion hieroglyphics were still in full use on the banks of the Nile.

The alphabet which had given such unhoped for results, whether applied to the great Obelisks at Karnac, or to other monuments which are also recognised as being of the age of the Pharaohs, presents to us the names of many other kings of this ancient race; the names of Egyptian deities; we can say more, substantives, adjectives, and verbs of the Coptic language: Young was then deceived when he regarded the phonetic hieroglyphics as a modern invention; when he advanced that they had served solely for the transcription of proper names foreign to Egypt. M. de Guiges, and above all M. Etienne Quatremère, established, on the contrary, a real fact and one of great importance,— that the reading of the inscriptions of the Pharaohs is corroborated by irresistible proofs, while they show that the existing Coptic language was that of the ancient subjects of Sesostris.

We now know the facts; I may then confine myself to confirm, by a few short observations, the consequences which appear to me to result from them.

Discussions of priority, even under the dominion of national prejudices, will have become embittered if they can be reduced to fixed rules, but in certain cases the first idea is everything; in others, the details offer the chief difficulties; sometimes the merit seems to consist less in the conception of a theory than in its demonstration. We then infer how much the choice of a particular point of view must depend on arbitrary conditions; and, lastly, how much influence it will have on the definitive conclusion. To escape from these embarrassments I have sought an example in which the parts respectively played by two rival claimants for

sound or the same articulation, can be substituted indifferently for each other. In the actual state of the Egyptian alphabet I perceive six or seven homophone signs for A, and more than twelve for the Greek sigma.—*Arago.*

an invention may be assimilated to those of Champollion and Young, and which has, on the other hand, united all opinions. This example, I believe, I have found in *the Interferences,* even leaving out of the question, as regards the subject of the hieroglyphics, the quotations from the memoir of M. de Guines. It is as follows: —

Hooke in fact had announced before Dr. Young that luminous rays interfered, just as the latter had asserted before Champollion that the Egyptian hieroglyphics are sometimes phonetic. Hooke did not prove directly his hypothesis; the proof of the phonetic values assigned by Young to different hieroglyphics could only rest on readings which had not as yet been made and which could not then be made.

From want of knowing the composition of white light, Hooke had not an exact idea of the nature of interferences, as Young on his part deceived himself by an imagined syllabic or disyllabic value of hieroglyphics.

Young, by unanimous consent, is regarded as the author of the theory of interferences. Thence, by a parity of reasoning which seems to me inevitable, Champollion ought to be regarded as the author of the discovery of hieroglyphics.

I regret not to have sooner thought of this comparison. If in his lifetime Young had been placed in the alternative of being the originator of the doctrine of interferences, leaving the hieroglyphics to Champollion, or to keep the hieroglyphics, giving up to Hooke the ingenious optical theory, I do not doubt he would have felt obliged to recognise the claims of our illustrious fellow-countryman. At all events there would have remained with him, what no one could have contested, the right to appear in the history of the memorable discovery of the interpretation of hieroglyphics in the same relative position as that in which Kepler, Borelli, Hooke, and Wren appeared in the History of Universal Gravitation.

NOTE.

We have here put before our readers the literal version of Arago's statement respecting the claims of Young in regard to the discovery of the principle of interpreting the Egyptian hieroglyphics. Arago's representations have been, as is well known, greatly called in question. And though he throughout speaks in a tone of marked courtesy and candour towards Young, yet it is clear that he espouses the cause of Champollion with an ardour which many, in this country, believe has, in some degree, blinded him to the truth of the case.

At any rate, in the vivid and highly coloured sketch here presented by M. Arago, the reader may need some caution in discriminating the fair share of merit which may be claimed by the respective parties engaged in the inquiry. The Author's national partialities may very naturally have had some influence in biassing his judgment. It is impossible here to enter on details of controversy. But both as to the actual amount and accuracy of Dr. Young's investigations and the relative claims of M. Champollion, the reader may find it desirable to refer to the extended discussion of the subject given in Dr. Peacock's *Life of Young.* Without the pretension, or, indeed, the possibility, of adequately going into this question within the limits of such a commentary as can be here given, we shall content ourselves with pointing out to the notice of our readers a few of those passages in that work in which Dr. Young's claims are so powerfully vindicated. The conclusions turn on such a variety of points of details that it would be wholly impracticable to attempt any analysis of them in this place. But the result tends to assign a considerably larger share of credit in the discovery to Dr. Young than Arago seems disposed to allow him. Dr. Peacock's able and elaborate work is doubtless in the hands of all those who take any interest in a question so important to the advance of philological and ethnological science as well as to general literature. Yet a slight sketch of the chief points referred to may not be useless.

We may first mention that Dr. Young's article "Egypt" in the *Supplement to the Encyclopedia Britannica,* published in 1819, contains the most comprehensive survey of his labours and conclusions on the subject of hieroglyphic literature up to that date. It does not profess to go into those minutiæ of critical detail, for which reference must be made to his numerous other writings on the subject. But as a general and popular view it will always be consulted with advantage. Nevertheless, the reader must always bear in mind that, in the statements thus given, much had to be revised, or even reversed, from the improved disclosures of his later researches.

Dr. Peacock has alluded but briefly to the views of Arago, and towards the conclusion of the chapter, sums up the representation of the case as given in the éloge, remarking only that the whole of his previous statements constitute the refutation of it.

The following extract will show the main claims of Young, insisted on by his biographer.

"It was Dr. Young who first determined, and by no easy process, that the 'rings'* on the Rosetta stone contained the name of Ptolemy. It was Dr. Young who determined that the semicircle and oval, found at the end of the second ring, in connexion with the former, was expressive of the feminine gender: and it was Dr. Young who had not only first suggested that the characters in the ring of Ptolemy were phonetic, but had determined, with one very unimportant inaccuracy, the values of four of those which were common to the name of Cleo-

* Certain portions of the hieroglyphical characters are found surrounded by a ring or enclosure called by the French "Cartouches."

patra, which were required to be analysed. All the principles involved in the discovery of an alphabet of phonetic hieroglyphics were not only distinctly laid down, but fully exemplified by him; and it only required the further identification of one or two royal names with the rings, which expressed them in hieroglyphics, to extend the alphabet already known sufficiently to bring even names which were not already identified under its operation."

Dr. Peacock states that Champollion and Young, while engaged simultaneously in the prosecution of the researches connected with these points, in some instances had opportunities of personal communication with each other. But Champollion enjoyed especial advantages from circumstances which placed some of the papyri in his possession; and thus enabled him to take precedence in the publication of results, while his competitor, if he had enjoyed the same facilities, would, no doubt, have been equally competent to perceive the force of the new evidence thus adduced, and equally ready to make use of it, even if setting aside some of his earlier inferences and conjectures.

Dr. Peacock, after reflecting with much severity on Champollion, expresses his regret to find so eminent a writer as Chevalier Bunsen, whose remarks are quoted before (p. 311.), "supporting, by the weight of his authority, some of the grossest of these misrepresentations" (p. 337.).

Dr. Young displayed singular modesty and forbearance in his controversy with Champollion, treating him throughout with all the respect due to his acknowledged eminence; and while mildly reproaching him with omitting to give him the due credit for his own share in the research, yet in no way insinuating that any discreditable motive led to the omission.

Dr. Peacock, however, thinks a far more stringent tone of criticism might have been fairly applied: he takes up the cause of Young with a less scrupulous zeal; and though with perfect good temper, yet with deeply damaging force of argument and statement of facts, exposes the very unjustifiable nature of Champollion's assumptions, and vindicates the claims of Young to his fair and important share in these discoveries.

He dwells on the tone of assumption in which Champollion presents himself to his readers as in exclusive possession of a province of which he had long since been the sole conqueror, and regards every question raised as to his exclusive rights as an unjustifiable attack to be resented and repelled: while he studiously suppresses the *dates* of the successive stages of the discovery, and thus attacks Young on the assertions made on imperfect knowledge in the earlier stages of his investigations, with the aid of all his own accumulated information acquired subsequently: a proceeding the iniquity of which needs only stating to stand exposed.

As instances of this, it is mentioned that Young, in 1816, on the strength of comparatively imperfect information then acquired, made some representations respecting the enchorial characters in the Rosetta inscription, and their relation to those employed in the funereal rolls. These Champollion criticises and exposes without reserve, from the

more full knowledge he had obtained in 1824; entirely passing over Young's own *later* statement on the same subject, correcting his former views, and from which even Dr. Peacock considers Champollion himself probably derived a large portion of his own knowledge of the subject!

Dr. Peacock has collected, in one point of view, Champollion's main assertions as representing the state of the case. But he has shown that some of the propositions dwelt upon were, in point of fact, *never maintained* by Dr. Young; and it was chiefly by his later researches, that the erroneous impressions at first entertained, respecting the points to which they relate, had been corrected, and their true nature established.

In 1821, Champollion denied altogether the existence of an alphabetic element among the hieroglyphics. But in the following year he adopted the whole of Young's principles, and applied them with one modification only. The analogy of certain marks in the Chinese hieroglyphics, to signify proper names, the principle that the phonetic power of the symbol is derived from the initial letter or syllable of the name of the object which it represents in the Egyptian language, are among the chief of those which he borrows without acknowledgment, or claims without regard to their prior announcement by Young. "It would be difficult," says Dr. Peacock, "to point out in the history of literature a more flagrant example of disingenuous suppression of the real facts bearing on an important discovery."—*Translator.*

MISCELLANEOUS WORKS OF DR. YOUNG.

The limits prescribed do not permit me even to quote the mere titles of all the numerous writings which Dr. Young published. Nevertheless, the public reading of so rich a catalogue would certainly have sufficed to establish the celebrity of our colleague. Who would not imagine in fact that he had before him the register of the labours of several academies, and not those of a single individual, on hearing, for instance, the following list of titles:

Memoir on the Establishments where Iron is wrought.
Essays on Music and Painting.
Remarks on the Habits of Spiders and the Theory of Fabricius.
On the Stability of the Arches of Bridges.
On the Atmosphere of the Moon.
Description of a new Species of Opercularia.
Mathematical theory of Epicycloidal Curves.
Restoration and Translation of different Greek inscriptions.
On the means of strengthening the Construction of Ships of the Line.
On the play of the Heart and of the Arteries in the phenomena of circulation.
Theory of Tides.
On the Diseases of the Chest.

On the Friction of the Axes of Machines.
On the Yellow Fever.
On the Calculation of Eclipses.
Essays on Grammar, &c.*

CHARACTER OF YOUNG. — HIS POSITION AS A PHYSICIAN. — HIS ENGAGEMENT ON THE NAUTICAL ALMANACK.— HIS DEATH.

Labours so numerous and varied seem as if they must have required the laborious and retired life of that class of men of science, which, to say the truth, is beginning to disappear, who from their earliest youth separate themselves from their companions to shut themselves up completely in their studies. Thomas Young was, on the contrary, what is usually called a man of the world. He constantly frequented the best society in London. The graces of his wit, the elegance of his manners, were amply sufficient to make him remarkable. But when we figure to ourselves those numerous assemblies in which fifty different subjects in turn are skimmed over in a few minutes, we may conceive what value would be attached to one who was a true living library, from whom everyone could find, at the moment, an exact, precise, substantial answer on all kinds of questions which they could propose to him. Young was much occupied with the fine arts. Many of his memoirs testify the profound knowledge which he had happily acquired of the theory of music. He carried out also to a great extent the talent of executing it: and I believe it is certain that of all known instruments, even including the Scottish bagpipe, only one or two could be named on which he could not play. His taste for painting developed itself during a visit which he paid to Germany. There the magnificent collection of Dresden absorbed his attention entirely; for he aspired not solely to the easy credit of connecting together, without mistake, the name of such or such an artist with such or such a painting; the defects and the characteristic qualities of the greatest masters, their frequent changes of manner, the material objects which they introduced into their works, the modifications which those objects and the colours underwent in progress of time, among other points, occupied him in succession. Young, in one word, studied painting in Saxony, as he had before studied languages in his own country, and as he afterwards studied the sciences. Everything, in fact, was a subject of meditation and research. The university cotemporaries of the illustrious phy-

* This list, it should be borne in mind, is intended by the author merely as a specimen of the vast catalogue which might be made of Young's writings; the reader will find ample details as to his innumerable productions in Peacock's *Life*.— *Translator*.

sicist recalled a laughable instance of this trait of his mind. They related that entering his room one day, when for the first time he had taken a lesson in dancing the minuet, at Edinburgh, they found him occupied in tracing out minutely with the rule and compasses, the route gone through by the two dancers, and the different improvements of which these figures seemed to him susceptible.

Young borrowed with happy effect, from the sect of the Friends, to which he then belonged, the opinion that the intellectual faculties of children differ originally from each other much less than is commonly supposed. "Any man can do what any other man has done," became his favourite maxim. And further, never did he personally himself recoil before trials of any kind to which he wished to subject his system. The first time he mounted a horse, in company with the grandson of Mr. Barclay, the horseman who preceded them leapt a high fence. Young wished to imitate him, but he fell at ten paces. He remounted without saying a word, made a second attempt, was again unseated, but this time was not thrown further than on to the horse's neck, to which he clung. At the third trial the young learner, as his favourite motto taught, succeeded in executing what another had done before him.* This experiment need not have been referred to here, but that it had been repeated at Edinburgh, and afterwards at Gottingen, and carried out to a further extent beyond what might seem credible. In one of these two cities Young soon afterwards entered into a trial of skill with a celebrated rope-dancer, in the other, (and in each case the result of a challenge,) he acquired the art of executing feats on horseback with remarkable skill, even in the midst of consummate artistes, whose feats of agility attract every evening such numerous crowds to the circus of Franconi. Thus, those who are fond of drawing contrasts may, on the one side, represent to themselves the timid Newton†, never riding in a carriage, so much did the fear of being upset preoccupy him, without holding to both the doors with extended arms, and, on the other, his distinguished rival, galloping on the backs of two horses with all the confidence of an equestrian by profession.

In England, a physician, if he does not wish to lose the confidence of the public, ought to abstain from occupying himself

* This anecdote seems at variance with what is stated on the authority of a Cambridge cotemporary of Young in Dr. Peacock's *Life* (p. 119.), that he only once there attempted to follow the hounds, when a severe fall prevented any further exhibitions of the kind. — *Translator*.

† This practice has been described as that of Newton, but the motive assigned by Arago is novel.

with any scientific or literary research which may be thought foreign to the art of curing diseases. Young for a long time did homage to this prejudice. His writings appeared under an anonymous veil. This veil, it is true, was very transparent. Two consecutive letters of a certain Latin motto served successively in regular order as the signature to each memoir. But Young communicated the three Latin words to all his friends both in his own country and abroad, without enjoining secrecy on any one.

Besides, who could be ignorant that the distinguished author or the theory of interferences was the Foreign Secretary of the Royal Society of London; that he gave in the Theatre of the Royal Institution, a course of lectures on mathematical physics; that, associated with Sir H. Davy, he published a journal of the sciences, &c.? and moreover, we must say that his anonymous disguise was not rigorously observed even in his smaller memoirs, and on important occasions, when, for instance, in 1807, the two volumes in quarto appeared of 800 or 900 pages each, in which all branches of natural philosophy were treated in a manner so new and profound, the self-love of the author made him forget the interests of the physician, and the name of Young in large letters replaced the two small Italics whose series was then terminated, and which would have figured in a rather ridiculous manner in the title-page of this colossal work.

Young had not then, as a physician, either in London or at Worthing, where he passed the sea-bathing season, any extended practice. The public found him, in fact, too scientific. We must also avow that his public lectures on medicine, those, for instance, which he delivered at St. George's Hospital, were generally but ill-attended. It has been said, to explain this, that his lectures were too dry, too full of matter, and that they were beyond the apprehension of ordinary understandings. But might not the want of success be rather ascribed to the freedom, not very common, with which Young pointed out the inextricable difficulties which encounter us at every step in the study of the numerous disorders of our frail machine.

Would any one expect at Paris, and especially in an age when every one seeks to attain his end quickly and without labour, that a professor of the faculty would retain many auditors if he were to commence with these words, which I borrow literally from Dr. Young: —

"No study is so complicated as that of medicine; it exceeds the limits of human intelligence. Those physicians who precipitately go on without trying to comprehend what they observe, are often just as much advanced as those who give themselves up to generalisations hastily made on observations in regard to which

all analogy is at fault." And if the Professor, continuing in the same style, should add, "In the lottery of medicine the chances of the possessor of ten tickets must evidently be greater than those of the possessor of five,"—when they believed themselves engaged in a lottery, would those of his auditors whom the first phrase had not driven away, be at all disposed to make any great efforts to procure for themselves more tickets, or to explain the meaning of our Professor — the greatest amount of knowledge possible?

In spite of his knowledge, perhaps even from the very cause that it was so extensive, Young was totally wanting in confidence at the bedside of the patient. Then the mischievous effects which might eventually result from the action of the medicine even the most clearly called for presented themselves in a mass to his mind; seemed to counterbalance the favourable chances which might attend the use of them; and thus threw him into a state of indecision, no doubt very natural, yet on which the public will always put an unfavourable construction.

The same timidity showed itself in all the works of Young which treated on medical subjects.* This man, so eminently remarkable for the boldness of his scientific conceptions, gives here no more than a bare enumeration of facts. He seems hardly convinced of the soundness of his thesis, either when he attacks the celebrated Dr. Radcliffe, whose whole secret in the most brilliant and successful practice was, as he has himself said, to employ remedies exactly the reverse of the usual way: or when he combats Dr. Brown, who found himself, as he says, in the disagreeable necessity of recognising, and that in accordance with the official documents of an hospital attended by the most eminent physicians, that on the average, fevers left to their natural course are neither more severe nor of longer duration than those treated by the best methods

In 1818, Young, having been named Secretary to the Board of Longitude, abandoned entirely the practice of medicine to give himself up to the close superintendence of the celebrated periodical work known under the name of the *Nautical Almanac.* From this date the *Journal of the Royal Institution* gave every quarter his numerous dissertations on the most important problems o

* This timidity in medical speculation is entirely borne out by the tenor c Young's intellectual character as exhibited in such forcible lineaments in th portrait presented to us by Dr. Peacock. His mind was essentially cast in matter of fact positive, demonstrative mould; hence all subjects of abstract c doubtful inquiry, in which probabilities alone could be estimated, or when th conclusions were to be the result of moral discrimination, were utterly unsuite to him. His medical character has been viewed, however, in a much highe light by Dr. Peacock, who has sought to combat the unfavourable impressio here advanced. See especially p. 213. and p. 222.— *Translator.*

navigation and astronomy. A volume entitled *Illustration of the Mecanique Céleste of Laplace*, a scientific discussion on the tides, amply attested that Young did not consider the employment he had accepted as a sinecure. This employment became nevertheless to him a source of unceasing disgust. The *Nautical Almanac* had always been from its commencement a work exclusively destined to the service of the navy. Some persons demanded that it ought to be made, besides, a complete astronomical ephemeris. The Board of Longitude, whether right or wrong, not having shown itself a strong partisan of the projected change, found itself suddenly the object of the most violent attacks. The journals of every party, Whig or Tory, took part in the conflict.

We were no longer to view it as a union of such men as Davy, Wollaston, Young, Herschel, Kater, and Pond, but an assembly of individuals (I quote the words), "who obeyed a Bœotian influence." The *Nautical Almanac*, hitherto so renowned, was now declared to have become an object of shame to the English nation. If an error of the press were discovered, such as there must be in any collection of figures at all voluminous, the British navy, from the smallest bark up to the colossal three-decker, misled by an incorrect figure, would all together be engulfed in the ocean, &c.

It has been pretended that the principal promoter of these foolish exaggerations did not perceive such serious errors in the *Nautical Almanac* until after he had unsuccessfully attempted himself to obtain a place in the Board of Longitude. I know not whether the fact was so. In any case, I would not make myself the echo of the malicious commentaries to which it gave rise: I ought not to forget, in fact, that for many years past that member of the Royal Society to whom I allude has nobly devoted a part of his large fortune to the advancement of science. This commendable astronomer, like all men of science whose thoughts are concentrated on one sole object, fell into the error, which I do not pretend to excuse, of measuring through a magnifying glass the importance of the projects he had conceived: but that with which above all he must be reproached is, that he did not foresee that the hyperbolic language of his attacks would be taken literally; that he forgot that at all epochs and in all countries there are a great number of persons who having nothing to console them for their littleness, seize, as a prey, on all occasions of scandal, and under the mask of zeal for the public good enjoy the delight of being ignoble defamers of those of their cotemporaries whose success has been proclaimed by fame. In Rome he whose office it was to insult the triumphant conqueror was altogether a slave; in London it was a member of the House of Commons

from whom the men of science received a cruel affront. An orator notorious for his prejudices, but who had hitherto vented his bitterness only against productions of French origin, attacked the most celebrated names in England, and retailed against them in open parliament, puerile accusations, with a laughable gravity. Ministers whose eloquence was exercised for hours on the privileges of a rotten borough, did not pronounce a single word in favour of genius. The Board of Longitude was suppressed without opposition. The next day, it is true, the wants of an innumerable marine service made their imperative voice heard, and one of the men of science who had been displaced, the former Secretary of the Board, Dr. Young, found himself recalled to his old labours. Paltry reparation! Would the man of science feel less the separation from his illustrious colleagues,—would the man of feeling less perceive that the noble fruits of human intellect were subjected to tariff by the representatives of the country, in pounds, shillings and pence, like sugar, pepper or cinnamon?

The health of our colleague, which had already become somewhat precarious, declined from this sad epoch with fearful rapidity. Skilful physicians by whom he was attended soon lost hope. Young himself had a consciousness that his end was approaching, and saw it come with an admirable calmness. Until his last hour he occupied himself without intermission on an Egyptian dictionary then in the press, and which was not published till after his death. When his powers did not permit him any longer to sit up, or to employ a pen, he corrected the proofs with a pencil. One of the last acts of his life was to exact the suppression of a small publication written with talent, by a friendly hand, and directed against all those who had contributed to the destruction of the Board of Longitude.* Young died surrounded by a family by

* The whole account of the transactions connected with the abolition of the Board of Longitude must be received with some qualification. Arago writes on the subject in his usual vehement tone, and in the feeling in which the whole affair would naturally be viewed by a foreigner, perhaps not intimately acquainted with the minute points of the case, and the somewhat different relative position occupied by the parties in England to that in which they might stand in France. It may be right very briefly to point out a few particulars in the case which are necessary for forming a correct impression of it. The Board of Longitude, originally instituted, as it name implied, for one specific object, which it was considered had been sufficiently attained, was in 1818 remodelled by Act of Parliament, when Dr. Young was appointed secretary to the Board and superintendent of the *Nautical Almanac*; the late Mr. F. Baily, whose eminence in astronomical science may perhaps be dated from that event, strongly pointed out the numerous defects of the *Nautical Almanac*; this led to some controversy of rather a sharp nature between himself and Dr. Young, who defended the existing system: other astronomers joined in the desire for these and even more extensive improvements, all which (with one slight concession) *were steadily*

whom he was adored, May 10, 1829, barely at the age of fifty-six. Examination showed that he suffered from ossification of the aorta.

opposed by Dr. Young. Among these advocates for reform were several members of the Board itself, who urged them at its meetings. There was also a very prevalent impression, even among its own members, that the Board was not well constituted, and might have been capable of much better service to the nation if its functions were less restricted and the selection of its members placed on a better footing. In other quarters impressions unfavourable to its utility were prevalent; and it can hardly be matter of surprise that when the Board was itself divided in opinion, the public or the legislature should entertain doubts of its utility, or even hostile feelings towards it. What were the precise notions of the government, or the machinations by which they were influenced, it is impossible to say: but it is certain that in 1828, chiefly through the influence of Mr. Croker, its dissolution was determined upon and carried by Act of Parliament without any opposition being attempted. Instead, however, of an enlarged Board with increased powers, three scientific advisers of the Admiralty were appointed, of whom Dr. Young was one, retaining the superintendence of the *Nautical Almanac:* a system which has been since remodelled in accordance with the report of a committee appointed out of the Astronomical Society.

Dr. Young appears all along to have been affected only by the personal acrimony of some of the attacks upon himself in relation to the editorship of the *Nautical Almanac*, and not at all by any feeling for the Board of Longitude, as Arago would regard it.

That Board, as already observed, was divided against itself, and it therefore fell. It was never upheld on the only right ground. Neither the Board nor the friends of science sufficiently urged the strong and irresistible claims which they might have preferred to the government of the country, that "*a council of science*," with extended powers, properly selected and adequately remunerated, would be the appropriate adjunct of the government of a country all whose resources are so powerfully developed in exclusive dependence on the applications of science.

The government would thus have had the means of sound scientific advice constantly at hand, of which experience proves they are in daily want on every emergency; and which they obtain by asking the *gratuitous* services of men of science, and the crown would have possessed the means of making a graceful acknowledgment of the services, and paying a just tribute to the genius, of men devoted to the higher branches of the abstract sciences, which are of a nature incapable in themselves of affording any kind of remuneration, or in the ordinary course leading to any of those honours or preferments which await eminence in other professions. — *Translator.*

The reader may be referred for details of the questions here considered to the following documents:

1. "Astronomical Tables and Remarks for 1822, published December 1821," by F. Baily, Esq., with "Remarks on the present defective state of the *Nautical Almanac*."
2. A Reply to these Remarks appeared in Mr. Brande's *Quarterly Journal of Science*, April 1822. (Attributed to Dr. Young.)
3. Practical Observations on the *Nautical Almanac*, &c., by Jas. South, F.R.S. 1822.
4. Reply to a Letter in the *Morning Chronicle* relative to the Government and Astronomical Science, &c. by the same. 1829.
5. Refutation of Misstatements &c., in a paper presented to the Admiralty by

I have not dwelt too long on the task imposed on me, if I have broughtout, as I wished to do, the importance and novelty of the admirable law of interferences. Young is now placed before your eyes as one of the most illustrious men of science in whom England may justly take pride. Your thoughts, anticipating my words, may perhaps perceive already, in the recital of the just honours shown to the author of so beautiful a discovery, the peroration of this historical notice. These anticipations, I regret to say, will not be realised. The death of Young has in his own country created very little sensation. The doors of Westminster* Abbey, so easily accessible to titled mediocrity, remained shut upon a man of genius, who was not even a baronet. It was in the village of Farnborough, in the modest tomb of the family of his wife, that the remains of Thomas Young were deposited. The indifference of the English nation for those scientific labours which ought to add so much to its glory, is a rare anomaly, of which it would be curious to trace the causes. I should be wanting in frankness, I should be the panegyrist not the historian, if I did not avow, that in general Young did not sufficiently accommodate himself to the capacity of his readers; that the greater part of the writings for which the sciences are indebted to him, are justly chargeable with a certain obscurity. But the neglect to which they were long consigned did not depend solely on this cause.

Dr. T. Young, and printed by order of the House of Commons, by the same. 1829.

6. Further Remarks on the present defective state of the *Nautical Almanac*, &c., by F. Baily, Esq., F. R. S., &c. 1829.
7. Report of the Committee of the Astronomical Society relative to the improvement of the *Nautical Almanac*, adopted by the Council of the Society and approved and ordered to be carried into effect by the Lords Commissioners of the Admiralty, 1830. Memoirs of Astronomical Society, vol. iv. p. 447.
8. A motion was made in the House of Commons, February 23, 1829, for certain Returns respecting the Board of Longitude and the *Nautical Almanac*, &c.

The Returns were made and printed consisting of (1) " A Memorandum of a Statement made to the Chancellor of the Exchequer for reforming the *Nautical Almanac*, and establishment of a new Board of Longitude. (2) A Paper read at the Board, by J. Herschel, Esq. (3) A Report on a Memorandum, &c. by Thomas Young, M.D. In the last Dr. Young makes answer to what he considers objections raised in the "Memorandum," and also replies to those of Mr. Baily and Mr. South. Sir J. South's Pamphlet contains the Memorandum,—the objections raised or inferred by Dr. Young — his replies to them — all which are severely criticised.

At p. 60. is a curious account of some discussions at Sir H. Davy's soirée between Sir J. South and Dr. Young.

* The frequenters of Poets' Corner need not be reminded that literature and science are not excluded from their share of funereal honours in Westminster

The exact sciences have an advantage over the works of art or imagination, which has been often pointed out. The truths of which they consist remain constant through ages without suffering in any respect from the caprices of fashion or the decline of taste: but thus, when once these researches rise into more elevated regions of thought, on how many competent judges of their merits can we reckon? When Richelieu let loose against the great Corneille a crowd of that class of men whom envy of the merit of others renders furious, the Parisians vehemently hissed the partisans of the despot Cardinal and applauded the poet. This reparation is denied to the geometer, the astronomer or the physicist, who cultivate the highest parts of science. Those who can competently appreciate them throughout the whole extent of Europe never rise above the number of eight or ten. Imagine these unjust, indifferent, or even jealous, (for I suppose that *may* sometimes be the case,) and the public, reduced to believe on hearsay, would be ignorant that D'Alembert had connected the great phenomenon of precession of equinoxes with the principle of universal gravitation; that Lagrange had arrived at the discovery of the physical cause of the libration of the moon; that since the researches of Laplace, the acceleration of the motion of that luminary is found to be connected with a particular change in the form of the earth's orbit, &c. &c. The journals of science, when they are edited by men of recognised merit, thus acquire, on certain subjects, an influence which sometimes becomes fatal. It is thus I conceive that we may describe the influence which the *Edinburgh Review* has sometimes exercised.

Among the contributors to that celebrated journal at its commencement, a young writer was eminently distinguished, in whom the discoveries of Newton had inspired an ardent admiration. This sentiment so natural, so legitimate, unfortunately led him to misconceive the plausible, ingenious, and fertile character of the doctrine of interferences. The author of this theory had not, perhaps, always taken care to clothe his decisions, his statements, his critiques, with those more polished forms of expression the claims of which ought never to be neglected, and which, moreover, became a matter of imperative duty when the question referred to the immortal author of the *Natural Philosophy** [the *Prin-*

Abbey. M. Arago here, as in some other passages, may naturally be a little incorrect in referring to national usages. The *delay* which occurred in regard to Young's monument, is however not fully explained by Dean Peacock. (See *Life of Young*, p. 485.) — *Translator.*

* It seems impossible to make this sentence intelligible unless we suppose the "immortal author" spoken of to be Newton, and by consequence that the title *Natural Philosophy* was a slip of the writer's pen, for *Principia.* Yet

cipia ?]. The penalty of retaliation was applied to him with interest: the *Edinburgh Review* attacked the man of erudition, the

the supposition that the hostility of the *Edinburgh Review* was at all called forth by any want of courtesy towards Newton in the writings of Young is wholly unsupported by anything in Young's papers, in which he cites the views of Newton with the greatest respect. — *Translator.*

Newton's support of the emission theory of light.— The authority of names can never be of any avail to the truly inductive philosopher — his motto is emphatically "nullius in verba." But there has been always a propensity among writers on the subject to dwell on such authority, and to array great names on either side of any of those controverted points which have divided the scientific world. Perhaps where the question is purely one of opinion and refers simply to hypotheses, upheld for what they are worth as such, the weight of a name may not be unworthy of due estimation: great experience and high genius may add value to a pure *hypothesis* though it could not to a positive *conclusion.* In regard to theories of light this has been conspicuously exemplified, and during a long continuance of controversial discussion it has been a matter of triumph to the opponents of the undulatory theory that the authority of Newton is on their side. And even Arago as well as some other supporters of it have spoken as if regretting that they were thus constrained to put themselves in antagonism to Newton. They have pictured two rival theories, the one headed by Newton and supported by Laplace, Biot, Brewster and Potter, the other upheld in opposition to them by Huyghens, Hooke, Euler, Young, Fresnel, Airy and all the Cambridge school.

But a very slight inquiry into the real facts entirely dispels this view of the case. In particular Dr. Young himself in proposing his theory, so far from opposing the Newtonian views, expressly endeavours to conciliate attention by claiming the weight of Newton's authority *on his own side:* thus in his paper "On the Theory of Light and Colours" (*Phil. Trans.* 1801,) he commences by highly extolling the optical researches of Newton, and then observes, "those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations (*i. e.* his own views) so much the more of their attention as they shall appear to coincide more nearly with Newton's opinion." He then proceeds to examine in detail a number of passages from Newton's writings in which the theory of waves is distinctly upheld and even applied with some precision to the explanation of various phenomena of light, illustrated by their analogies to those of sound.

It is perfectly true that Newton in the actual investigation of several phenomena of light adopts other hypotheses than those of waves: and chiefly the idea of light (whatever may be its nature) being subject to certain attractions and repulsions, — to certain bendings when approaching near the edges of solid bodies,— to certain peculiar modifications or changes in its nature recurring periodically at certain minute intervals along the length of a ray, —to the idea of a ray having "sides" endued with different properties; in a word, a variety of conceptions, which he introduces for the purpose of giving some kind of imaginary physical representation of the *modus operandi* in each of the several curious experimental cases which he had examined. In all these there is no unity or community of principle, there is at least nothing like the spirit of theory no continual recurrence to one leading idea,—no perpetual appeal to any one principle however imaginary, but an attempt in each isolated case to frame something like an isolated hypothesis to suit it, and in some way to represent its phenomena though without any attempt to connect them with the others

writer, the geometer, the experimenter, with a vehemence, with a severity of expression almost without example in scientific discussion. The public usually keeps on its guard when such violent

It may perhaps be said that all these various suppositions agree in supposing light to be material, to be something emitted from the luminous source. But on a closer examination it seems far from certain that even this can be maintained. The only part of these investigations, perhaps, in which anything very positive of this kind is distinctly introduced, is when Newton investigates the laws of refraction, on the express supposition of small molecules attracted by the molecules of the medium. But in this instance it has been truly observed, that at the time when Newton wrote, no mathematical method existed by which this kind of action could be reduced to calculation except those involving the action of attractive force. To give, then, a mathematical theory of ordinary reflexion and refraction he was necessitated to make use of this method. When he came to investigate those more recondite phenomena which he (very appropriately to their *apparent* nature) called "inflexion," the idea most naturally and obviously presented was, that some power or influence, analogous to attraction and repulsion, existing in the edge of an opaque body to bend out of their course rays passing very near it, and this might seem to imply the materiality of those rays. A kind of *alternating* action of this sort, which he imagined necessary to account for a part of the effect, would, however, hardly be reconcileable to the idea of direct emission. It would be a difficult matter to conceive particles darted through space with such inconceivable velocity as must belong to those of light, and yet stopping to wave about, in and out, as Newton expresses it, "like an eel," close to the edge of a body, by virtue of some mysterious influence which it exercises upon them.

Again: the theory of those alternating states, conditions, or "fits" as he termed them, at such minute intervals along the length of ray alternately putting it in a state to be reflected, and again to be transmitted by a transparent medium, seem very remote from the idea of a simple rectilinear progress of molecules through space following one another at immense intervals of distance though in inconceivably rapid succession in time. It would be easy to extend such remarks; but it will probably be seen, with sufficient evidence for our present purpose, that neither in profession nor in fact, can Newton's name be appealed to as at all an exclusive supporter of the material hypothesis of light; even if in other passages he had not distinctly referred to that of undulations. And of these references a large number are quoted from different portions of his writings, by Dr. Young, in the paper above cited. In some of these, while he admits the readiness with which the idea of waves represents the phenomena, he yet dwells on certain apparent objections which seemed to invalidate that idea.

Upon the whole it appears that the name of Newton can in no way be legitimately claimed as a partisan of either theory. Indeed, it is surprising that any claim of the kind could have been set up as regards the emission theory after his own distinct avowal: —

"'Tis true that from theory I argue the corporeity of light: but I do it without any absolute positiveness, as the word 'perhaps' intimates; and make it at most but a very plausible consequence of the doctrine, and not a fundamental supposition, nor so much as any part of it." — *Phil. Trans.* vol. x. 1675, p. 5086.

While in respect to either hypothesis it is sufficiently evident to those acquainted with his writings that he never *systematically* upheld either the one or the other: but from time to time, as each particular investigation seemed to require, he adopted the one or the other principle just as it seemed to give the more ready explanation of the point before him.—*Translator*.

language is addressed to it, but in this instance they adopted at the first onset the opinions of the journalist in which we cannot fairly accuse them of inconsiderateness. The journalist, in fact, was not one of those unfledged critics whose mission is not justified by any previous study of the subject. Several good papers, received by the Royal Society, had attested his mathematical knowledge, and had assigned him a distinguished place among the physicists to whom optical science was indebted: the profession of the bar in London had acknowledged him one of its shining luminaries: the Whig section of the House of Commons saw in him an efficient orator who in parliamentary struggles was often the happy antagonist of Canning: this was the future President of the House of Peers, — the present Lord Chancellor.* How could opposition be offered to unjust criticisms proceeding from so high a quarter? I am not ignorant what firmness some minds enjoy in the consciousness of their being in the right; in the certainty that sooner or later truth will triumph: but I know also, that we shall act wisely in not reckoning too much on such exceptions. Listen, for example, to Galileo himself, repeating in a whisper after his abjuration, "E pur si muore!" and do not seek in these immortal words an augury for the future, for they are but the expression of the cruel vexation which the illustrious old man experienced. Young also, in writing a few pages which he published as an answer to the *Edinburgh Review*, showed himself deeply discouraged. The vivacity, the vehemence of his expressions, ill concealed the sentiment which oppressed him. In a word, let us hasten to say that justice, complete justice, was at length rendered to the great physicist. After several years the whole world recognised in him one of the brightest luminaries of the age. It is from France (and Young took pleasure in himself proclaiming it) that the first sign of this tardy reparation showed itself. I will add, that at an epoch considerably before the doctrine of interferences had made converts either in England or on the Continent, Young found within his own family circle one who comprehended it, and whose assent to it might well console him for the neglect of the public. The distinguished person whom I here point out to the notice of the physicists of Europe, will excuse me if I complete this indiscretion by stating the circumstances.

In the year 1816 I made a tour in England with my scientific friend M. Gay-Lussac. Fresnel had just then entered on his scientific career in the most brilliant manner, by the publication of his memoir on Diffraction. This work which, in our opinion, contained a capital experiment irreconcileable with the Newtonian

* Lord Brougham, who held that office when this biography was written.

theory of light, became naturally the first subject of our discussion with Dr. Young. We were astonished at the numerous qualifications which he put upon our praises of it, until at length he stated to us that the very experiment which we so much commended had been published, so long since as 1807, in his treatise on Natural Philosophy. This assertion did not seem to us well founded. It caused a long and minute discussion. Mrs. Young was present, without appearing to take any part in the conversation; but we imagined that the weak fear of being designated by the ridiculous sobriquet of bas-bleu rendered the ladies of England very reserved in the presence of foreigners; and our want of discernment did not strike us till the moment when Mrs. Young quickly quitted her place: we then began to attempt excuses to her husband, until we saw her re-enter the room carrying under her arm a large quarto volume. This was the first volume of the *Natural Philosophy*. She placed it on the table, and without saying a word opened it at page 787., and pointed with her finger to a diagram in which the curvilinear route of the diffracted bands, on which the discussion turned, was theoretically established.

I trust I shall be pardoned these little details. Too numerous examples may almost have habituated the public to consider destitution, injustice, persecution, and misery as the natural wages of those who devote their vigils to the development of the human mind! Let us not then forget to point out the exceptions whenever they present themselves. If we wish that youth should give itself up with ardour to intellectual labours, let us show them that the glory attached to great discoveries allies itself, sometimes at least, with some degree of tranquillity and happiness. Let us even withdraw, if it be possible, from the history of science so many pages which tarnish its glory. Let us try to persuade ourselves that in the dungeons of the Inquisitors, a friendly voice had caused Galileo to hear some of the delightful expressions which posterity has kept sacred for his memory; that behind the thick walls of the Bastille, Fréret might yet have learned from the world of science, the glorious rank which it had reserved for him among the men of erudition whom France honours; that before going to die in an hospital, Borelli had found sometimes in the city of Rome a shelter against the inclemency of the atmosphere, and a little straw on which to lay his head; and lastly, that the great Kepler had not experienced the sufferings of hunger.

NOTE BY THE AUTHOR.

The Journals having done me the honour to mention sometimes the numerous testimonies of good-will and friendship which Lord Brougham had shown me in 1834, as well in Scotland as in Paris, a word or two of explanation here seem indispensable. The éloge of Dr. Young was read at a public sitting of the Academy of Sciences, Nov. 26. 1832. At this period I had never had any personal acquaintance with the writer in the *Edinburgh Review*, and thus all charge of ingratitude must fall to the ground. But could you not, some might perhaps say, have suppressed entirely, when your paper was going to the press, all that related to so unfortunate a controversy? I could have done so, and in fact the idea had occurred to me: but I soon renounced it. I know too well the elevated feelings of my illustrious friend to fear that he will take offence at my frankness in regard to a question on which I have a profound conviction that the great extent of his genius has not preserved him from error. The homage which I render to the noble character of Lord Brougham in now publishing this passage of the éloge of Young without any modification, is, in my mind, sufficiently significant to render it needless to add a word more.

JAMES WATT.

BIOGRAPHY READ AT THE PUBLIC MEETING OF THE ACADEMY OF SCIENCES, ON THE 8TH OF DECEMBER, 1834.

GENTLEMEN,— After having waded through a long list of battles, assassinations, plagues, famines, catastrophes of all sorts presented by the annals of I know not what country, a philosopher exclaimed, "Happy the nation whose history is tedious!" Why ought we to add, in a literary point of view at least, "Unhappy the man on whom the duty falls to relate the history of a happy people!"

If the philosopher's exclamation loses none of its appositeness when applied to mere individuals, its counterpart characterises with equal truth the position of some biographers.

Such were the reflections that occurred to me, whilst I was studying the life of James Watt, and collecting obliging communications from the relations, friends, and companions of the illustrious mechanic. His life, quite patriarchal, devoted to work, to study, and to meditation, will not afford us any of those striking events the recital of which, sprinkled with judgment among scientific details, relieves their weight. Still I will relate it, if but to show in what a humble position those projects were perfected, that were destined to raise the British nation to an unheard-of degree of power. I will especially endeavour to characterise, with extreme precision, the fruitful inventions which will for ever connect the name of Watt with the steam-engine. I foresee all the dangers of this line of conduct; I am aware that it may be said on going out of this room: We expected an historical eulogy, but we have only received a dry and arid *lesson.* Besides this, the reproach would not have weighed on me, if the *lesson* had been well understood. I will, therefore, exert every effort not to tire your patience; I will keep in mind that clearness is politeness in public speakers.

INFANCY AND YOUTH OF JAMES WATT. — HIS ADVANCEMENT TO THE APPOINTMENT OF ENGINEER TO THE UNIVERSITY OF GLASGOW.

James Watt, one of the eight Foreign Associates of the Academy of Sciences, was born at Greenock in Scotland, the

19th of January, 1736. Our neighbours on the other side of the Channel, have the good sense to think that the genealogy of a respectable and industrious family, is quite as worthy of being preserved as the parchments of certain titled families, that have become celebrated only by the enormity of their crimes and their vices. Thus I can say with certainty that the great grandfather of James Watt was an agriculturist, settled in the county of Aberdeen; that he was killed in one of Montrose's battles; that the conquering side, as was customary (I was going to add, as is still customary in civil discords), did not think death itself a sufficient expiation for the opinions in support of which the poor farmer had fought; that it punished the act in the person of the son, by confiscating his property: that the unfortunate child, Thomas Watt, was received by some distant relations; that in the entire insulation to which his difficult position condemned him, he assiduously devoted himself to deep studies; that in more tranquil times, he settled at Greenock, where he taught mathematics and the elements of navigation; that he resided at Crawford's Dyke, of which borough he was magistrate; and that finally he died in 1734, at the age of eighty-two.

Thomas Watt had two sons. The eldest, John, followed his father's profession at Glasgow. He died at the age of fifty (1737), leaving a chart of the Clyde*, which was published under the care of his brother James. This James, who was the father of the celebrated engineer, and for a long time treasurer of the municipal council of Greenock, as well as magistrate of the town, became remarkable in the performance of his duties by his ardent zeal, and an enlightened spirit of amelioration. He *combined* (do not be alarmed: these three syllables, that have become a subject of general anathema in France, will not injure the memory of James Watt), he *combined* three species of occupation: he was at once a seller of all sorts of nautical instruments† and stores, a builder, and a merchant; which unfortunately, about the close of his life, did not prevent certain commercial speculations from depriving him of a portion of the creditable fortune that he had gained before. He died at the age of eighty-four, in 1782.

James Watt, the subject of this essay, was born with a very

* This map is re-engraved in the *Memorials of Watt*, with an advertisement which ascribes its publication to James Watt at Glasgow College; a MS. note on one copy, said to be in the handwriting of the Great Engineer, states that it was published by John Watt in 1760. — *Translator*.

† It may have been first owing to an examination of these instruments, that young Watt, in his eighteenth year, in conformity with his own desire, was apprenticed to a mathematical instrument-maker in London. — *Translator*.

delicate constitution. His mother, whose maiden name was Muirhead, gave him his first instruction in reading. He learned writing and ciphering from his father. He also attended the *Grammar School* of Greenock; and thus these humble Scotch seminaries are entitled, with just pride, to enrol the name of this celebrated engineer among the pupils that they have formed; as the College of La Flèche boasted of Descartes, as the University of Cambridge still cites Newton.

To be correct, I must add that frequent indispositions prevented young Watt from punctually attending the public school at Greenock; that during a great portion of the year he was confined to his room, and there devoted himself to study, without any out-door help. As is frequently the case with high intellectual faculties destined to yield great results, they began to develope themselves in retirement and solitude.

Watt was too sickly for his parents to think of urging him to assiduous occupation. They even left his amusements to his free choice. We shall see whether he abused this freedom.

A friend of Mr. Watt's one day found little James lying on the floor, and with a piece of chalk drawing all sorts of intersecting lines: whereupon he exclaimed — "Why do you allow that child to waste his time — send him to the public school!" Mr. Watt answered: "You might have spared us this hasty judgment; before condemning us, examine attentively what our son is doing." The apology soon followed: the boy, only six years old, was seeking the solution of a geometrical problem.

Prompted by an enlightened fondness, the father had early furnished the young scholar with a certain number of tools, and he made use of them with great ability; he took to pieces and put together again all the infantine toys that came into his hands; he continually made new ones. When older, he applied them to the construction of a small electrical machine; the bright sparks from which became a lively subject of amusement and surprise to all the playfellows of the poor invalid.

Watt, with an excellent memory, still would not perhaps have figured among the young prodigies of common schools; he would have refused to learn lessons like a parrot, because he felt an internal longing carefully to elaborate the intellectual elements which they presented to his mind. Nature had especially created him for meditation. The father, moreover, augured very favourably of the rising faculties of his son. Other less observant relations did not participate in these hopes: his grandmother, Mrs. Muirhead, said to him one day, — "James, I never saw such an idle young man as you are. Do take a book, and employ

yourself usefully. Upwards of half an hour has elapsed without your saying a single word. Do you know what you have been doing all this time? You have taken off and replaced, and taken off again, the teapot lid; and you have alternately held in the steam that came out, first a saucer and then a spoon; you have busied yourself in examining and collecting together the little drops formed by the condensation of the steam, on the surface of the china and of the silver: is it not disgraceful to waste your time in this manner?"

In 1750, each of us in Mrs. Muirhead's position would probably have held the same language; but the world has advanced, and general knowledge has advanced with it; also, when I shall have presently explained, that the principal discovery of our associate consisted in a special manner of transforming steam into water, the reproaches of Mrs. Muirhead will appear quite in a different light; and little James watching the tea-pot, will be the great Engineer anticipating the important discoveries that were to immortalise him; every one will then perceive how remarkable it was that the words *condensation of steam* should so naturally have entered into an account of Watt's early childhood. Independently of this, I could not but think, from the singularity of the anecdote, that it deserved to be preserved. When an opportunity offers, let us prove to young people that Newton was not diffident only on that day when, to satisfy the curiosity of a high personage who desired to know how attraction had been discovered, he answered, *By constantly thinking of it!* Let us show to everybody, in the simple words of the immortal author of the *Principia*, the real secret of men of genius.

The love of anecdote that our associate showed so agreeably during upwards of half a century to all those around him, developed itself very early. The proof will be found in some lines that I am about to quote and translate from an unedited note given in 1798, by Mrs. Marion Campbell, a cousin and juvenile companion of the celebrated engineer.*

"During a journey to Glasgow, Mrs. Watt entrusted her young son James to one of her friends. After a few weeks she returned to see him, but most assuredly not expecting the reception she met with. "Madam," said her friend, as soon as she saw her, "you must hasten to take James back to Greenock. I can no

* I am indebted for this curious document to my friend Mr. James Watt, of Soho. Thanks to the deep veneration that he feels for the memory of his illustrious father, and thanks to the inexhaustible complaisance with which he listened to all my inquiries, I have been able to avoid several errors that have slipped into the most esteemed biographies, and from which I myself, deceived by verbal references too easily accepted, should otherwise have fallen into.

longer endure the state of excitement into which he throws me: I am harassed by want of sleep. Every night, when the usual hour approaches for the family to retire to bed, your son adroitly contrives to raise a discussion, in the course of which he always finds means to introduce a story; this story is sure necessarily to engender a second, and a third, &c. And these tales, whether they be pathetic or comic, are so charming, so interesting, that the whole of my family listen to them with such attention, that a fly might be heard to fly. Thus hours follow hours, without our being aware of it; but the next day I am ready to drop with fatigue. Dear madam, take your son home."

James Watt had a younger brother, John*, who by deciding to follow his father's profession, left James the liberty of choosing any vocation; for according to Scotch customs, it suffices if one son adopts the paternal career. But it was difficult to say what vocation this would be, for the young student seemed to succeed equally well in whatever he tried.

The shores of Loch Lomond, already so celebrated by the reminiscences that they afford of the historian Buchanan, and of the illustrious inventor of Logarithms, developed his taste for the beauties of nature and for botany. Excursions to various mountains in Scotland made him feel that the inert crust of the globe is not less worthy of attention, and he became a mineralogist. James availed himself also of his frequent intercourse with the poor inhabitants of those picturesque districts, to learn their local traditions, their popular ballads, and their ignorant prejudices. When ill health confined him to his paternal roof, chemistry became the principal object of his experiments. Gravesande's *Elements of Natural History* initiated him into the thousands of wonders in general physics; finally, like all invalids, he devoured all the works on medicine and surgery that he could obtain. These latter sciences had awakened such a passion in the student, that he was detected one day carrying into his room the head of a child who had died of an unknown malady, for the purpose of dissecting it.

Still, Watt did not decide either in favour of botany, of mineralogy, of literature, of poetry, of chemistry, of physics, or of surgery, although he was so well prepared for each of those studies. In 1755 he went to London to place himself under Mr. John Morgan, a maker of mathematical and nautical instruments, in Finch Lane, Cornhill. The man, who was to cover England with motive powers by the side of which, as to their

* He was lost in 1762, in one of his father's ships, on her passage from Greenock to America, aged twenty-three years.

effects at least, the old colossal machine of Marly would seem a mere pygmy,—entered on the manual art of constructing with his own hands subtile, delicate, fragile instruments, those small but admirable reflecting sextants, to which nautical art owes its progress.

Watt did not remain above a year at Mr. Morgan's, and returned to Glasgow, where some heavy difficulties awaited him. Attached to their old privileges, the corporations of arts and of trades regarded the young artist from London as an intruder, and obstinately refused him permission to open even the most humble workshop. Every means of conciliation failing, the University of Glasgow interfered, and ceded to young Watt a small locality in their own buildings, allowing him to open a shop there, and honouring him moreover with the title of their Engineer.* There still exist some small instruments of that epoch, of exquisite workmanship, made entirely by Watt's own hands. I will add that his son has lately placed before me the first essays of a steam-engine, and that they are truly remarkable by the high finish of the work, the firmness, the precision of the form. It was not then without reason, whatever may have been said of it, that Watt spoke with complacency of his own manual dexterity.

Perhaps you have some reason to think, that I carry my scruples rather far, in claiming for our associate a species of merit which cannot add to his glory. But I will acknowledge that I never intend to make a pedantic enumeration of the qualities with which superior men have been endowed, without recollecting that wretched general in the age of Louis XIV. who always carried one shoulder very high, because Prince Eugene of Savoy was rather deformed, and thought that this sufficed without his endeavouring to extend the likeness any farther.

Watt had scarcely reached his one and twentieth year when the University of Glasgow attached him to their establishment. He had had as protectors, Adam Smith, author of the celebrated work on *The Wealth of Nations*; Black, whose discoveries relative to latent heat and carbonate of lime must place him in a distinguished rank among the most eminent chemists of the eighteenth century; Robert Simson, the celebrated restorer of the most important treatises by ancient geometers. These great men, however, thought at first that they had only delivered a good,

* This was not all. According to Stuart's Narrative, Watt picked up a practical acquaintance with machines from an industrious mechanic at Glasgow; a person "who was by turns a cutler and a whitesmith, a repairer of fiddles, a tuner of spinets, and a mender of fishing-tackle,"—in a word, a very useful man at almost everything.—*Translator.*

zealous and amiable workman from the overbearing corporations; but they soon after recognised in him a first-rate man, and bestowed on him their warmest friendship. The students in the University also esteemed it an honour to be admitted into Watt's intimacy. In short *his shop*, yes, Gentlemen, his shop! became a sort of academy, where the illustrious men of Glasgow used to go to discuss the most delicate questions of art, of science, and of literature. I would not dare, I own, to pronounce what share this young workman of one and twenty took in these learned circles, if I could not depend on an unpublished paper by the most illustrious of the editors of the *British Encyclopedia.*

Robison says, " Although still a student, I had the vanity to think myself sufficiently advanced in my favourite subjects, mechanics and physics, when I was introduced to Watt; and I will acknowledge that I was not slightly mortified when I perceived how far superior the young workman was to me. . . . Whenever any difficulty arrested us in the University, we used to run to our workman. When once excited, any subject became for him a text for serious study and discoveries. He never let go his hold, until he had entirely cleared up the proposed question; he either reduced it to nought, or obtained from it some net and substantial result. . . . One day, the desired solution seemed to require that Leupold's work on Machines should be read: Watt immediately learned German. On another occasion, and for a similar reason, he rendered himself master of the Italian language. . . . The ingenuous simplicity of the young engineer immediately procured for him the good will of all who accosted him. Although I have lived much in the world, I must assert, that it would be impossible for me to cite a second example of so sincere and so general an attachment felt for a man of uncontested superiority. It is true that this superiority was veiled by the most amiable candour, and allied to a firm resolution to recognise every man's merit liberally. Watt was even inclined to assign things to the inventive spirit of his friends that were often only his own ideas dressed up in another form. I have all the more reason to assert his possessing this rare disposition, because I have myself experienced the effects of it."

You will have to decide, Gentlemen, whether it was not equally honourable to pronounce these closing words, as to have inspired them.

The deep and varied studies to which the circumstances of his singular position gave rise, never interfered with the young artist's professional work. He attended to this in the daytime; and devoted the night to theoretical researches. Relying on the resources of his imagination, Watt seemed to delight in difficult

enterprises, and those to which he seemed least adapted. Will it be believed that he undertook to construct an organ, he who was totally insensible to the charms of music; he who had never been able even to distinguish one note from another, as *g* from *f*? Yet this enterprise was successfully carried out. We need scarcely add, that the new instrument presented essential improvements in the mechanical parts, in the regulators, in the method of appreciating the power of the wind; but it will excite astonishment to learn that its harmonic qualities were not less remarkable, and that they charmed professional musicians. Watt solved an important part of the problem: he arrived at the temperament assigned by a man learned in the mystery, in aid of the phenomenon of the vibrations then but ill understood; and which he could not have dived into, but in the profound though obscure work of Dr. Smith *, of Cambridge.

PRINCIPLES OF THE STEAM-ENGINE.

I have now reached the most brilliant portion of Watt's life, and also, I fear, the most difficult part of my task. The immense importance of the inventions of which I am about to treat, cannot be doubted for one moment; but I may not succeed in rendering them suitably appreciated, without entering into intricate numerical comparisons. In order that these comparisons, if they become essential, may be easily seized, I will present you, in the most compact manner possible, with the delicate notions of physics on which we shall have to rest them.

By the effect of simple changes of temperature, water may exist in three perfectly different conditions; in the solid state, in the liquid state, and in the aërial or gaseous state. Below zero on the scale of the centigrade thermometer, water becomes ice; at 100° it rapidly assumes the form of gas; in all the intermediary temperatures it remains fluid.

Careful observation of the instant of change from one of these conditions to another, leads to discoveries of the highest order, and which are keys to the economical appreciations of steam-engines.

Water is not necessarily hotter than any kind of ice; water may be kept at the temperature of zero without freezing; ice may remain at zero without melting; but while this water and this ice are both of the same temperature, are both at zero, it seems difficult to believe that they do not differ but by their physical properties; that no element, extraneous to the water so-

* Dr. Robert Smith's work, here alluded to, was intituled *Harmonics, or the Philosophy of Musical Sounds*, and printed in 1760. He was also the author of the well-known *System of Optics*. — *Translator*.

called, distinguishes the solid water from the fluid. A very simple experiment will clear up this mystery.

Mix a kilogram of water at zero with a kilogram of water at 79° centigrade; the two kilograms of the mixture will be at a temperature of 39° and a half; that is to say, at the mean of the two constituent fluids. The hot water preserves 39° and a half of its former heat, and has ceded 39° and a half to the cold water; this is very natural and might have been foreseen.

But let us repeat this experiment with one modification: instead of the kilogram of water at zero, let us take a kilogram of ice at the same temperature of zero. From the admixture of this kilogram of ice with the kilogram of water at 79°, there will result two kilograms of fluid water, because the ice bathed in the hot water cannot fail to melt and to preserve its former weight; but do not hastily attribute to the mixture, as in the preceding instance, a temperature of 39° and a half; for you would be mistaken; the temperature will be only zero; there will be no trace left of the 79° of heat which the hot water possessed: those 79° disintegrated the molecules of ice, they may have combined with them, but without heating them at all.

I do not hesitate to represent this experiment of Black's as one of the most remarkable in modern physics. Indeed, see the consequences to which it leads.

Water at zero, and ice at zero, differ in their internal constitution. The fluid contains, beyond what the solid body does, 79° of an imponderable body called *caloric.* Those 79° are so well hidden in the composition, — I was almost going to say in the aqueous amalgam, that the most sensitive thermometer does not reveal their existence. Heat then, imperceptible to our senses, imperceptible to the most delicate instruments, LATENT *heat,* for that is the name given to it, is one of the constituent principles of those bodies.

The comparison of boiling water, of water at 100°, with the vapour that rises from it, and the temperature of which is also 100°, leads, though on a much grander scale, to analogous results. At the moment of the formation of steam of the temperature of 100°, the water at the same time imbibes an enormous quantity of heat in a *latent* form, in a form not sensible to the thermometer. When the steam resumes its fluid form, this latent heat is disengaged, and heats all bodies it meets with capable of absorbing it. If, for example, we occasion 5·35 kilograms of water at zero to be traversed by a single kilogram of steam at 100°, the steam will be entirely liquified. The 6·35 kilograms resulting from the mixture will be of the temperature of 100°. In the composition of one kilogram of steam then, a quantity of latent heat is absorbed tha

would raise a kilogram of water, provided the evaporation was prevented, from zero to 535° of the centigrade scale. This result will undoubtedly appear enormous, but it is certain; the steam of water is created only on these conditions. Wherever a kilogram of water at 100° evaporates naturally or artificially, it must appropriate to itself, in order to be transformed, and it does attract from the surrounding bodies, 535° of heat. And these degrees, it cannot be too often repeated, are integrally restored by steam on whatever surfaces it is subsequently liquified. This ingenious proceeding is very ill understood, if it is supposed that the aqueous gas carries through the tubes where it circulates only the heat that is thermometrically sensible: the principal effects are due to the constituent heat, the hidden heat, the latent heat, which disengages itself at the moment when a contact with cold surfaces restores the steam from its gaseous to its fluid state.

Henceforward, then, we must place heat among the constituent principles of the steam of water. Heat is obtained only by burning wood or coal; steam therefore has a commercial value superior to that of water, by all the price of the combustible used in the act of creating steam. If the difference of the two values is very great, it must be attributed to the latent heat; for the thermometric or sensible heat has but a small share in it.

I may perhaps have occasion to enlarge, in the sequel, on some other properties of the steam of water. If I do not mention them at this moment, it is not that I attribute to this assembly the state of mind of certain scholars, who said one day to their Professor of Geometry — "Why do you take so much trouble to demonstrate these theorems? We place entire confidence in you; give us your *word of honour* that it is true, and nothing more need be said!" But it is my duty not to abuse your patience; I have to keep in mind also, that by referring to special treatises, you can easily fill up the lacunæ that I have unavoidably left.

HISTORY OF THE STEAM-ENGINE IN ANCIENT TIMES.

Let us now endeavour to take the part of those nations and individuals, who appear to deserve to be quoted in the history of the steam-engine. Let us trace the chronological series of improvements made in this machine, from its first germs, which were very ancient, up to the discoveries by Watt. I enter on the subject with a firm inclination to be impartial, with a strong desire to render to each inventor the justice that is due to him, with a certainty of continuing a stranger to any consideration unworthy of the mission that you have given me, or unworthy of the majesty of science, or arising from national prejudices. I ac-

knowledge, on the other hand, that I shall pay but little attention to the numerous decisions already come to, dictated by similar prejudices; that I shall allow myself still less, if possible, to be influenced by the bitter criticisms that no doubt await me, for the past is the mirror of the future.

A question well expressed is half solved. If this sensible axiom had been attended to, the discussions relative to the invention of the steam-engine would not assuredly have presented the symptoms of acrimony and violence with which they have been impressed till now. But the authors had blindly plunged into a defile that had no outlet, by wanting to find a sole inventor, in a case that requires us to distinguish several. The best informed watch-maker of the history of his art would be struck dumb, if he were asked in general terms, who was the inventor of watches? Though the question would not embarrass him much, if it referred separately to the moving power, to the various forms of escapement, or to the balance. And so it is with the steam-engine: it now presents the realisation of several capital ideas, but each quite distinct, that could not have proceeded from one and the same source, and of which it is our duty carefully to seek out the origin and date.

If having made any use whatever of steam, as it has been asserted, gave a right to figure in its history, we ought to quote the Arabs in the first place; because from time immemorial, their principal food, the flour of maize, which they call *couscoussou* *, has been cooked by steam in cullenders placed over rustic boilers. Such an instance suffices to show up the ludicrous nature of the principle whence it results.

Our countryman Gerbert, the same who wore the tiara as Sylvester II., does he acquire more real claims when, about the middle of the 9th century, he made the tubes of an organ in the Cathedral of Rheims resound by means of steam from water? I think not: in the embryo Pope's instruments I perceive a current of steam substituted for a current of common air, to produce the usual musical phenomenon from the organ pipes; but by no means a mechanical effect, properly so called.

The first example of motion generated by steam is to be found

* This *kùskùs*, or *cuscasou*, is a very nutritious dish; it consists of corn paste crumbled and put into an earthen cullender over a boiling pot in which meat or fowls, with ochra (*pisum ochrus*) and other vegetables, are stewing; and which is luted or stopped close round the junction. The contents of the cullender are therefore dressed by steam. How ancient this mode of cooking may be we know not, but the Arabs only go back to the flight in A.D. 622; about which time, as tradition has it, it was invented by Mahomet when his health required wholesome and savoury food.— *Translator.*

in a toy still older than Gerbert's organ; in an eolipyle by Hero of Alexandria, the date of which is as far back as 120 B.C. Perhaps it may be difficult, not possessing any figure, to give a clear idea of the mode of action in this little apparatus; still I will attempt it.

When gas escapes in a certain direction from the vase in which it is contained, this vase by means of re-action will be inclined to move in a diametrically opposite direction. The recoil of a gun loaded with gunpowder is a similar agency: the gas engendered by the combustion of the saltpetre, the charcoal, and the sulphur, rushes into the air according to the direction of the gun; the re-action, produced in the rear, reaches the shoulder of the person who fires it; it is on the shoulder then that the recoil must act. To change the direction of the recoil, it would suffice to make the jet of gas issue in another direction. If the gun were closed at the end, and were pierced only by a lateral opening horizontally perpendicular to its direction, the gas from the powder would escape laterally and horizontally; therefore the recoil would act perpendicularly to the direction of the gun; and its force would be exerted on the arms and not on the shoulder. In the former instance, the recoil pushed the man who fired from the front to the rear, as if to upset him; in the second instance it would tend to turn him round on his own axis. Let the gun then be invariably fixed, and horizontally, to a vertical moveable axis, and at the instant of being fired, it will alter its direction more or less, and it will also make the axis turn.

Continuing the same arrangements, let us suppose the rotatory vertical axis to be hollow, but closed at its upper end; let it rise from the base as a sort of chimney from the cauldron where gas is engendered; let there be besides a free lateral communication between the interior of this axis and the interior of the gun's barrel, so that, after having filled the interior of the axis, the steam enters the gun barrel, and issues by the lateral horizontal opening. Except as to intensity, this steam in escaping will act like the gases disengaged from the powder in the barrel of the gun when closed at the end, and pierced laterally; only, we should not have but a mere shock, as occurred in the instance of the sudden and harsh explosion from the gun; on the contrary, the rotatory motion will be uniform and continuous, like the cause which engenders it.

Instead of only one gun, or rather instead of merely one horizontal tube, let several be adapted to the vertical rotatory tube and we shall have, with the exception of some slight differences the ingenious apparatus of Hero of Alexandria.

Here we should, doubtless, have a machine in which the steam

of water creates motion, and may produce mechanical effects of some importance; it would be truly a steam-engine. But let us hasten to add that it would have no real point of contact, either in its shape, or the moving power's mode of action, with the machines of that kind now in use. If ever the reaction of a current of steam becomes practically useful, we must, without hesitation, refer the idea back to Hero; though at present the rotatory eolipyle can only be quoted here as carving on wood is cited in the history of printing.*

HISTORY OF THE STEAM-ENGINE IN RECENT TIMES.

In our manufacturing machines, in our packet-boats, in our railways, the motion results directly from the *elasticity* of steam. It is therefore of importance to seek where and how the idea of this power first arose.

Neither the Greeks nor the Romans were ignorant that the steam of water can acquire a prodigious mechanical power. They already explained, by the aid of the sudden evaporation of a certain quantity of this fluid, the frightful earthquakes which in a few seconds hurl the ocean beyond its natural limits; which overturned from their very foundations the most solid monuments of human labour; which suddenly create dangerous rocks in the midst of deep seas; and which heave up high mountains in the very centre of continents.

Whatever may be said of it, this theory of earthquakes does not show that its authors had devoted themselves to appreciations, to experiments, to exact measurements. No one now ignores that at the moment when the incandescent metal flows into the founder's moulds of earth or plaster, a few drops of any fluid contained in them would suffice to occasion dangerous explosions. Notwithstanding the progress of science, modern founders do not always avoid such accidents: how then could the ancients entirely keep clear of them? Whilst they cast thousands of statues, splendid ornaments of their temples, of their public places, of their gardens, of the private dwellings of Athens and of Rome, misfor-

* These reflections are applicable also to the project that Branca, an Italian architect, published at Rome in 1629, in a work entitled *Le Machine*, and which consisted in generating a rotatory movement by directing the steam issuing out of an eolipyle as breathings, or in a current, or on the pallets of a wheel. If, contrary to all probability, steam is some day usefully employed in the simple form of blowing, Branca, or the author, now unknown, from whom he may have borrowed the idea, will take the first rank in the history of this new species of machines. Relative to the machines of the present day, the claims of Branca would be quite null.

tunes must have occurred: the artisans discovered the immediate cause; the philosophers, on the other hand, following out the spirit of generalisation, which was the characteristic of their schools, saw in them miniatures, or true images, of Etna's eruptions.

All this may be true, without having the least importance in the history that now occupies us. I have not even insisted, I acknowledge, so much on these slight lineaments of ancient science relative to the power of steam, but in order to live at peace, if possible, with the Daciers of both sexes, with the Dutens of our own epoch.*

Both natural and artificial powers, before becoming really subservient to the use of man, have almost always been adopted for objects of superstition. Nor will the steam of water be an exception to the general rule.

The chronicles informed us that on the banks of the Weser, the God of the ancient Teutones sometimes indicated his displeasure to them by a sort of thunder-clap, which was immediately followed by a cloud that filled the sacred area. The image of their God Bustérich, found, it is said, in some excavations, clearly reveals the way in which the pretended prodigy was obtained.

The statue was of metal. The hollow head contained an amphora of water. Wooden plugs closed the mouth and a hole above the forehead. Some charcoal, adroitly placed in the cavity of the cranium, gradually warmed the fluid. Soon the steam that was generated made the tompions fly out with a great report: then it escaped in two violent jets, and formed a thick cloud between the God and his stupified worshippers. It would appear that in the middle ages some monks thought the invention a good prize, and that the head of Bustérich has not acted before assembled Teutones only.†

* For the same reason, I cannot dispense with here relating an anecdote which, notwithstanding its romantic style, and containing what we now know to be contrary to the way in which steam acts, still shows the high opinion that the ancients had formed of this mechanical agent. It is related that Anthemius, Justinian's architect, occupied a house next door to that of Zeno, and to annoy that orator, who was his declared enemy, he placed in the ground floor of his own house several cauldrons containing water; that from an opening made in the lid of each of these there proceeded a flexible tube, which was conducted into the party-wall and up to the beams that supported the floors in Zeno's house; in short, that those floors heaved as if there had been a violent earthquake, as soon as fire was applied to the boilers.

† Hero of Alexandria attributed the sounds, the objects of so much controversy, issuing from the statue of Memnon when the rays of the rising sun shone upon it, to the passage, through certain openings, of a current of steam that the solar heat was deemed to have produced at the expense of the fluid with which the Egyptian priests, it is said, provided the interior of the pedestal of the Colossus. Solomon de Caus, Kircher, &c., have gone so far as to wish to dis-

In order to meet with useful notions on the properties of steam after the first glimpses given by the Greek philosophers, we are obliged to pass over an interval of twenty centuries. It is true that then some precise, conclusive, and irresistible experiments follow upon conjectures devoid of proofs.

In 1605, Flurence Rivault, gentleman of the bed-chamber to Henry IV., and preceptor to Louis XIII., discovers, for example, that a shell or bomb, if made thick and containing water, when placed on the fire after *being well closed,* (so as to prevent the steam from expanding freely in the air in proportion as it is generated,) will sooner or later explode. The power of steam is here found characterised by a clear and, to a certain degree, sensitive proof, with numerical appreciations; but it is still presented to us as a terrible means of destruction.*

Some eminent minds did not stop short at this meagre reflection. They conceived that mechanical forces, as well as human passions, must become useful or injurious, according as they are well or ill directed. In the special instance of steam, the simplest arrangement suffices to adapt this redoubtable elastic power to productive labour; which, according to all appearances, shakes the earth from its very foundations, which surrounds the furnace of the statuary with real dangers, and which shatters the thickest bomb into a hundred fragments!

In what state does this projectile exist before its explosion? The lower part contains some very hot water, *but still fluid;* the rest of the cavity is filled with steam. This steam, it being the characteristic trait of all gaseous substances, exerts its action equally in every direction: it presses with similar intensity on the water and on the metallic envelope that contains it. Let us apply a tap to the lower part of the bomb. As soon as it is open, the water, being urged by the steam, will rush out with extreme swiftness. If the tap opens into a tube, which, after curving round the

cover the special arrangements by which the theocratic fraud took possession of credulous imaginations; but all these suppositions lead us to think that they have not guessed right, even if there be, in this respect, anything to guess.

* If any learned man were to think that by stopping at Flurence Rivault I have not gone back far enough; if he were to borrow a quotation from Alberti, who wrote in 1411; if, according to that author, he were to tell us that from the beginning of the fifteenth century, the lime-burners feared much for themselves and their kilns, from the explosion of pieces of limestone fortuitously containing some cavities, I should answer that Alberti himself was ignorant of the real cause of those terrible explosions, for he attributed them to the *air* in the small cavities being transformed into *steam* by the action of flame. I must remark, in conclusion, that a piece of limestone, accidentally hollow, would not have afforded the means of numerical appreciation of which Rivault's experiment seems susceptible.

exterior of the shell, rises up vertically, the water, being forced to change its course, will rise up in the tube in proportion to the elasticity of the steam; or, for it is the same thing though in other words, the water will rise in proportion to its temperature. The only limits of the ascending movement will be the resistance of the walls of the apparatus.

For our bomb let us substitute a thick metal caldron, of vast capacity, and nothing will prevent our carrying large quantities of fluid to indefinite heights by the simple action of steam; we shall have constructed, in the full meaning of the word, a steam-engine capable of emptying or exhausting.

You now know the invention which France and England have disputed upon, as formerly seven towns of Greece claimed respectively the honour of having been the cradle of Homer. On the other side of the Channel the honour is assigned to the Marquis of Worcester, of the illustrious house of Somerset. On this side of the Straits, we feel that it belongs to a humble engineer*, almost entirely forgotten in biographical works: to Solomon de Caus, born at Dieppe, or in its environs. Let us cast an impartial glance on the claims of these two competitors.

Worcester, deeply implicated in the political intrigues of the latter years of the reigns of the Stuarts, was confined in the Tower of London.

> "Que faire en pareil gîte, à moins que l'on ne songe?"
> What could we do in such a bed but dream?

Now one day, according to tradition, the lid of the saucepan, in which his dinner was being cooked, suddenly rose. Worcester then considered the strangeness of the phenomenon that he had just witnessed. The thought now occurred to him that the same power which had raised the lid might become, under certain circumstances, a useful and convenient motive power. After recovering his liberty, he described, in 1663, in a book entitled *Century of Inventions*†, the means by which he intended to realise

* The term "un humble ingénieur" is hardly applicable, for De Caus was, before the year 1612, engineer and architect to Louis XIII., King of France; he then entered the service of the Elector Palatine, who married the daughter of James I., with whom he came to England, and was employed by the Prince of Wales in ornamenting Richmond Gardens. His work was entitled *Les Raisons des Forces Mouvantes, avec diverses Machines tant utiles que plaisantes.* In Partington's Lectures on the Steam Engine, he quotes a book by *Isaac* De Caus, "natif de Dieppe;" it is named *Nouvelle Invention de lever l'Eau plus haut que sa source, avec quelques Machines mouvantes par le moyen de l'Eau;* it is a folio volume without date or place.— *Translator.*

† It is expressly stated on the title-page of this pamphlet, that it was written in the year 1655, though not published till 1663.— *Translator.*

his idea. The essential part of these means, as far at least as they can be understood, is the bomb half filled with a fluid, and the vertically ascending tube, as we just now described.

This bomb, this same tube, are drawn in the *Reason of Moving Forces,* a work by Solomon de Caus. There, the idea is presented clearly, simply, and without any pretension. Its origin has nothing romantic in it; it is not connected with the events of civil war, nor with a celebrated state prison, nor even with the rising of the lid of a prisoner's saucepan; but, what is of infinitely more importance in a question of priority, it is, by its publication, forty-eight years older than the *Century of Inventions,* and forty-one years anterior to Worcester's imprisonment.

Thus reduced to a comparison of dates, the debate seemed to be brought to a close. Indeed, how maintain that 1615 had not preceded 1663? But those persons whose principal aim was to expel any French name from this important chapter of the history of the sciences, immediately changed their ground, as soon as they had occasioned the *Reasons of Moving Forces* to be disinterred from their dusty shelves. Without hesitation they pulled down their former idol; the Marquis of Worcester was sacrificed to the desire of annulling the claims of Solomon de Caus; the bomb placed on a burning brazier with its ascending tube, ceased at last to be the true germ of the present steam-engine! *

As to myself, I do not grant that the man was of no utility who, reflecting on the enormous elasticity of steam when greatly heated, was the first to perceive that it might be used to raise great quantities of water to all imaginable heights. I cannot admit that no mention is due of the engineer who was also the first to describe a machine adapted to realising such effects. Let us not forget that we cannot judge soundly of the merit of an invention, without transferring ourselves in imagination to the epoch in which it was made; without expelling from our mind for the time all the knowledge that subsequent centuries have poured in upon us. Let us imagine an ancient mechanic, Archimedes, for example, consulted on the means of raising the water contained in a vast, closed, metallic recipient, to a great height. He would certainly speak of great levers, of single or multiplied pulleys, of a windlass, perhaps of his ingenious screw; but what would be his surprise if

* In *Les Raisons des Forces Mouvantes*, it is evident that De Caus ascribes the force which shivered the shell entirely to the air; and he seems to consider that the force of the air proceeded from the water which exhaled in it. M. Arago cannot be borne out in saying, of those who do not arrive at his conclusions, that "*d'écarter tout nom Français*" was their principal thought. We know not to whom he alludes in assigning such a base motive, but the assertion infringes greatly on the impartiality which he promised us. — *Translator.*

to solve the problem some one would be content with a faggot and a match? Well, I must ask, should we dare to refuse the epithet of invention to a proceeding at which the immortal author of the earliest and true principles of statics and hydrostatics would have been astonished? The apparatus of Solomon de Caus, that metallic envelope in which an almost indefinite motive power is created by the aid of a faggot and a match, will always figure nobly in the history of the steam-engine.*

It is very doubtful whether Solomon de Caus, or Worcester, ever had their apparatus made. This honour belongs to an Englishman †, to Captain Savery.‡ I compare the machine constructed by that engineer in 1698, to that of his predecessors, although he introduced some essential modifications into it; that, amongst others, of generating the steam in a separate vessel. Although it may signify little relative to principle whether the

* It has been printed that G. B. Porta had given in his *Spiritali*, in 1606, nine or ten years before the publication of Solomon de Caus's work, the description of a machine intended to raise water by means of the elastic power of steam. I have elsewhere shown that the learned Neapolitan does *not speak, either directly or indirectly, of a machine* in the passage alluded to; that his aim, that his only aim, was to determine experimentally the relative volumes of water and of steam; that in the small physical apparatus employed for this purpose, according to the very words of the author, the steam could not raise the water more than a few centimetres (some inches); that in the whole description of the experiment, there is not a single word implying the idea that Porta knew the power of this agent, and the possibility of applying it to the production of a useful machine.

Is it thought that I ought to have quoted Porta, at least on account of his researches on the transformation of water into steam? But I should then say that the phenomenon had already been studied with attention by Professor Besson of Orleans, about the middle of the sixteenth century, and that one of the treatises of that mechanic in 1569, contains a special essay on determining the relative volumes of water and steam.

† Bonnain says that, after Kircher's death, a model was found in his museum of a machine which that enthusiastic writer had described in 1656, and which differed from that of Solomon de Caus only in one respect — the motive steam was engendered in a vessel totally distinct from that containing the water to be elevated.

‡ Thomas Savery was a sailor, but, not being in the Royal Navy, is styled Esquire Savery in the Royal Society correspondence. Nor is our author quite right in supposing this was the first engine. The Marquis of Worcester did actually make an "apparatus" to *drive water up by fire;* and Cosmo de Medici Grand Duke of Tuscany, describes in his *Diary* that he saw it in operation at Vauxhall, in the year 1656, — "went beyond the palace of the Archbishop of Canterbury, to see an hydraulic machine invented by my Lord Somerset Marquis of Worcester. It raises water more than forty geometrical feet by the power of one man only; and in a very short space of time will draw up four vessels of water through a tube or channel not more than a span in width." Savery's engine was an improvement upon this by the introduction of a vacuum — *Translator.*

steam be generated from the same water that is to be elevated and in the same caldron where it is to act, or from a separate vessel to be admitted at will through a tube of communication furnished with a tap above the fluid that is to be expelled, it is not so in practice. Another alteration, still more useful and worthy of special mention, equally due to Savery, will be more appropriately treated of in the article that we shall devote in the sequel to the labours of Papin and Newcomen.

Savery had entitled his work *The Miner's Friend.* But the miners did not show themselves sensible of his politeness. With only one exception, none of them ordered his machine. They have been used only to distribute water through various parts of palaces and country houses, parks and gardens; recourse has been had to them only to overcome a difference of levels of twelve or fifteen metres. We must keep in mind, however, that the danger of explosion would have been very great, if the immense power had been given to this apparatus that the inventor asserted they could bear.

Notwithstanding the incompleteness of Savery's practical success, this engineer's name deserves to hold a very distinguished place in the history of the steam-engine. Those persons whose life has been devoted to speculative exertions, are little aware of the distance there is between the project, apparently the most studied, and its realisation. Not that I presume to say, with a celebrated German Professor, that Nature always exclaims *no, no!* when we wish to raise a corner of the veil with which she covers herself; but, in following up the same metaphor, it may be permitted to assert that the enterprise increases in difficulty, in delicacy, and in uncertainty, in proportion as it requires the united efforts of more artists, and the employment of more material elements; in these various respects, and considering the nature of the epoch, no one can have felt himself more unfavourably situated than Savery.

MODERN STEAM-ENGINE.

I have spoken hitherto of steam-engines, the resemblance of which to those that now bear that name may be more or less contested. We shall now treat of the *modern steam-engine,* of that which is in use in our manufactories, in our boats, at the entrance of nearly all our wells and mines. We shall see it created, then enlarge and develop itself, sometimes by the inspiration of clever men, sometimes by the prickings of necessity, for necessity is the mother of genius.

The first name that we shall meet in this new period is that of

Denis Papin. It is to Papin that France will owe the honourable rank that she may claim in the history of the steam-engine. Stil the highly legitimate pride, which these successes inspire us with will not be unmixed. We shall find the claims of our countrymen nowhere but in foreign collections; his principal works will be published beyond the Rhine; his liberty will be menaced by the revocation of the edict of Nantes; it will be in a painful exile that he will temporarily enjoy that privilege of which studious men are most jealous, tranquillity of mind. Let us hasten to throw a veil over the deplorable results of our civil discords; let us forget that fanaticism was attached to the religious opinions of the physicist of Blois and return to mechanics; in this respect at least the orthodoxy of Papin has never been disputed.

There are two things to be considered in every machine; on one hand the moving power, on the other the arrangement, more or less complicated, of the fixed and moveable parts, and by the aid of which the moving power transmits its action to the resistance. At the height to which mechanical knowledge has now attained, the success of a machine intended to produce great effects, depends chiefly on the moving power, on the way of applying it, and economising its action. And therefore it was to produce an economical moving power capable of making the piston of a large cylinder oscillate constantly, that Papin devoted his life. Then to obtain from the oscillations of the piston the power required to turn the stones of a corn mill, the cylinders of a flatting mill, the paddle-wheels of a steamboat, the bobbins of a spinning mill: to raise the heavy hammer that strikes with redoubled blows enormous masses of incandescent iron, on its coming out of the reverberating furnace; to cut thick bars of metal with the two jaws of the shears, as we cut ribbon with sharp scissors: those are, I repeat it, so many problems of a very secondary order, and that would not puzzle the commonest engineers. We may employ ourselves, therefore, exclusively with the means by the aid of which Papin proposed to induce his oscillating motion.

Let us imagine a large vertical cylinder, open at top, and its base resting on a metallic table, pierced with a hole that a cock can either close or open at will.

Introduce a piston into this cylinder, that is to say, a circular plate, filling it entirely but moveable, that shall exactly close it. The atmosphere will rest with all its weight on the upper surface of this species of diaphragm, and will push it from the top to the bottom. The portion of atmosphere that occupies the lower part of the cylinder will tend, by reaction, to produce an inverse movement. This second force will be equal to the first if the tap is open, because gas presses equally in all directions. The piston will thus

find itself urged by two opposite forces, which will equalise each other. It will descend, however, though only by reason of its gravity. A counterpoise, slightly heavier than the piston, will suffice to raise it, on the contrary, up to the summit of the cylinder, and keep it there. Suppose the piston to have reached this extreme position. Let us seek the means to make it descend from thence *with great force*, and carry it up again.

Let us imagine that after having shut the lower cock, we succeed in *suddenly* annihilating all the air contained in the cylinder, —in a word, to render it a vacuum. A vacuum once made, the piston not receiving any pressure but from the external atmosphere which presses it from above, *will descend rapidly*. On this movement being achieved, the cock will be opened. The air will thereby return underneath to counterbalance the upper atmosphere. As at the beginning, the counterpoise will make the piston remount to the cylinder, and all the various parts of the apparatus will be found in their initial state. A second evacuation, or, if we like it better, a second annihilation of the internal air, will again make the piston fall, and so on.

The true moving power in this arrangement would be the weight of the atmosphere. Let us hasten to undeceive those who would think that they found in the facility with which we walk, and even run through the air, an index of the weakness of this motive power. With a cylinder of two metres in diameter, the effort made by the piston of the pump in descending, the weight that it could raise to an equal height with the cylinder at each of these oscillations, would be 35,000 kilograms. This enormous power, frequently renewed, may be obtained by a very simple apparatus, if we discover a prompt and economical method of alternately generating and destroying the atmospheric pressure at will, in a metal cylinder.

This problem was solved by Papin. Its beautiful and great solution consists in substituting an atmosphere of steam for the common atmosphere; by replacing the latter with a gas which at 100 centigrade degrees has exactly the same elastic force, but with the important advantage, not possessed by the atmosphere, that the power of aqueous gas is very soon destroyed on its temperature being lowered, that it ends by almost entirely disappearing if sufficiently cooled. I should equally well characterise Papin's discovery, and in few words, if I said, that he proposed to use the steam of water to make a vacuum in large spaces; and that this is besides a prompt and economical method.*

* An English engineer, deceived no doubt by some imperfect translation, asserted not long since that the idea of employing the steam of water in one and the same machine, as an elastic power and as a rapid means of forming a

The machine in which our countryman was the first to combine the elastic force of steam with the property possessed by this vapour of annihilating itself by cooling, he never made on a large scale. His experiments were always made with simple models. The water intended to generate the steam was not even contained in a separate vessel; enclosed in the cylinder, it rested on the metal plate that closed the orifice at the bottom. It was this plate that Papin heated directly, to transform the water into steam; it was from the same plate that he took away the fire when he wished for condensation to be effected. Such a proceeding, barely allowable in an experiment intended to verify the correctness of a principle, would evidently be still less admissible if the piston were required to move with some celerity. Papin, whilst saying that success might be attained "by various constructions easy to imagine," does not indicate any of them. He leaves to his successors both the merit of applying his fruitful idea, and that of inventing the details, which alone could ensure the success of the machine.

In our early researches respecting the employment of steam, we have had to quote—ancient philosophers of Greece and Rome; one of the most celebrated mechanics of the Alexandrian school; a pope; a gentleman of the court of Henri IV.; a hydraulist, born in Normandy (in that province fruitful in great men, that has enriched the national Pleiad with Malherbe, Corneille, Poussin, Fontenelle, Laplace, and Fresnel); a member of the House of Lords; an English engineer; finally, a French doctor, of the Royal Society of London *; for, we must acknowledge it, Papin, almost always exiled, was only a correspondent of our Academy. Now, however, simple artisans, mere workmen, will enter the list. All classes of society will thus have concurred towards the creation of a machine by which the whole world was to benefit.

In 1705, fifteen years after the publication of Papin's first memoir in the *Acts of Leipzig*, Newcomen and Cawley, one of

vacuum, belonged to Hero. On my side I have proved, unanswerably, that the Alexandrian mechanic had never thought of steam; that in his apparatus the alternate movement was to result only from the dilatation and the condensation of air, arising from the intermittent action of the solar rays.

* The ingenious Dr. Denis Papin was intimately connected with the Royal Society and its illustrious president, Newton, since he held the office of curator to that body, on a salary of forty pounds per annum. It is to be regretted that the funds of the Society, then, were so low that some of Papin's offered experiments seem to have hung fire, on account of the expenses amounting to fifteen pounds! Newton reported favourably on the proposal; and Papin said, "I am fully persuaded that Esquire Savery is so well-minded for the public good that he will desire, as much as anybody, that this may be done." It is a singular incident in the history of the wonderful engine, that though Papin invented the safety-valve, he did not apply it to his steam-machine.—*Translator.*

them a hardware man, the other a glazier at Dartmouth, in Devonshire, constructed (be pleased to observe that I did not say projected, for the difference is great), constructed a machine intended to effect drainage, and in which there was a separate caldron for generating the steam. This machine, as well as Papin's small model, has a vertical metal cylinder, closed at the bottom, open at the top, with a well-adjusted piston, intended to travel from end to end, both rising and falling. Both in the one apparatus and in the other, when the steam can freely reach the base of the cylinder, fill it, and thus counterbalance the pressure of the external atmosphere, the ascending movement of the piston is effected by a counterpoise. In the English machine, in short, in imitation of Papin's, as soon as the piston has reached its maximum of ascent, the steam that had tended to push it there is refrigerated; thus a vacuum is created throughout the space that the piston had traversed, and the external atmosphere forces it to descend again.

To obtain the requisite refrigeration, we already know that Papin contented himself with removing the chafing-dish that heated the base of the little metallic cylinder. Newcomen and Cawley had recourse to an arrangement far preferable in every respect: they poured an ample quantity of cold water into the circular space contained between the outer surface of the cylinder of their machine and the internal surface of a second, rather larger cylinder, which served as an envelope to the first. The cold was gradually communicated through all the thickness of the metal, and lastly reached the steam itself.*

Papin's machine, thus perfected as to the manner of cooling or condensing the steam, excited the proprietors of mines to the highest pitch. It spread rapidly through several counties of England, and rendered great service. The slowness of its movements, the necessary consequence of the tardiness with which the steam cooled and lost its elasticity, was still a subject of great regret. Chance fortunately indicated a very simple way of obviating this impediment.

At the beginning of the eighteenth century the art of casting metallic cylinders, and of hermetically sealing them by means of moveable pistons, was still in its infancy. Wherefore in Newcomen's early engines, the piston was covered with water intended to fill up the vacancies between the circumference of this moveable

* Savery had already adopted throwing cold water on the *exterior surface* of a metal vase, to condense the steam within it. This was the origin of his partnership with Newcomen and Cawley; yet we must not forget that Savery's patent, his machines, and the work in which he describes them, are posterior by several years to Papin's memoirs.

piece and the surface of the cylinder. To the great surprise of the manufacturers, one of their engines began one day to oscillate much faster than usual. After many examinations it was ascertained that on that day the piston was pierced, that some cold water fell into the cylinder by little drops, and that by passing through the steam, they annihilated it rapidly. From this fortuitous incident may be dated the entire abandonment of all exterior refrigeration, and the adoption of a watering pot, to shed a *shower of cold water* throughout the capacity of the cylinder at the instant of the piston's descent. The alternate up and down motion now acquired all the desired swiftness.

But let us see whether there was not another equally important improvement effected also by chance.

Newcomen's first engine required the most uninterrupted attention from the person who had to open and shut the cocks either to introduce the steam into the cylinder, or to throw in the cold shower intended to condense it. It happened on a certain day that this person was young Henry Potter.* The companions of this child were then at play, and their exclamations of joy tantalize him severely. He longs to go and join them; but the task entrusted to him would not allow of half a minute's absence. His mind becomes excited; strong passion awakes his genius: he discovers relative connections of which he had never dreamt before. Of the two taps, the one was to be opened at the moment when the beam, that Newcomen had been the first to introduce to such good purpose in his engines, had completed the descending oscillation, and it must be shut exactly at the opposite oscillation. The management of the second tap must be the exact contrary. The positions of the balance and of the taps are necessarily dependent on each other. Potter seizes on this fact: he perceives that the beam may impress on the other pieces all the motion that the play of the engine required, and instantly realises his conception. Several cords are fastened to the handles of the taps; the opposite ends Potter ties to portions of the beam suitably selected; thus the purchase which this exercises on certain cords while rising, and those that it exercises on others in descending, supplant the manual efforts; for the first time the engine works by itself; for the first time no other workman is seen near it but the stoker, who from time to time goes to keep up the fire under the boiler.

For the cords of young Potter, the constructors soon substituted rigid vertical rods fixed to the beam and furnished with several pegs which pressed the heads of the several taps or valves either

* The name of this play-loving and ingenious boy appears to have been *Humphrey* Potter.— *Translator.*

downwards or upwards. The rods themselves have been exchanged for other combinations; but however humiliating such an acknowledgment may be, all these inventions are mere modifications of the mechanism suggested to a child by the wish to join his little companions at play.

WATT'S LABOURS IN THE STEAM-ENGINE.

In physical cabinets we find a good many machines on which industry had founded great hopes, though the expense of their manufacture, or that of keeping them at work, has reduced them to be mere instruments of demonstration. This would have been the final fate of Newcomen's machine, in localities at least not rich in combustibles, if Watt's efforts, of which I must now present you with an analysis, had not come in to give it an unhoped-for degree of perfection. This perfection must not be considered as the result of some fortuitous observation, or of a single inspiration of genius; the inventor achieved it by assiduous labour, by experiments of extreme delicacy and correctness. One would say that Watt had adopted as his guide that celebrated maxim of Bacon's — "To write, speak, meditate, or act, when we are not sufficiently provided with *facts* to stake out our thoughts, is like navigating without a pilot along a coast strewed with dangers, or rushing out on the immense ocean without compass or rudder."

In the collection belonging to the University of Glasgow, there was a little model of a steam-engine by Newcomen that had never worked well. The Professor of Physics, Anderson, desired Watt to repair it. In the hands of this powerful workman the defects of its construction disappeared; from that time the apparatus was made to work annually under the inspection of the astonished students. A man of common mind would have rested satisfied with this success. Watt, on the contrary, as usual with him, saw cause in it for deep study. His researches were successively directed to all the points that appeared likely to clear up the theory of the machine. He ascertained the proportion in which water dilates in passing from a state of fluidity into that of vapour; the quantity of water that a certain weight of coal can convert into vapour; the quantity and weight of steam expended at each oscillation by one of Newcomen's engines of known dimensions; the quantity of cold water that must be injected into the cylinder to give a certain force to the piston's descending oscillation; and finally the elasticity of steam at various temperatures.

Here was enough to occupy the life of a laborious physicist yet Watt found means to conduct all these numerous and difficult researches to a good termination, without the work of the shop

suffering thereby. Dr. Cleland wished, not long since, to take me to the house, near the port of Glasgow, whither our associate retired on quitting his tools, to become an experimenter. It was razed to the ground! Our anger was keen but of short duration. Within the area, still visible of the foundations, ten or twelve vigorous workmen appeared to be occupied in sanctifying the cradle of modern steam-engines; they were hammering with redoubled blows various portions of boilers, the united dimensions of which certainly equalled those of the humble dwelling that had disappeared there. On such a spot, and under such circumstances, the most elegant mansion, the most sumptuous monument, the finest statue, would have awakened less reflection than those colossal boilers.

If the properties of steam are still present to your mind, you will perceive at a glance, that the economic working of Newcomen's engine seems to require two irreconcileable conditions. When the piston descends, the cylinder is required to be cold, otherwise it meets some steam there, still very elastic, which retards the operation very much, and diminishes the effect of the external atmosphere. Then, when steam at the temperature of 100° flows into that same cylinder and finds it cold, the steam restores its heat by becoming partially fluid, and until the cylinder has regained the temperature of 100°, its elasticity will be found considerably attenuated; thence will ensue slowness of motion, for the counterpoise will not raise the piston until there is sufficient spring contained in the cylinder to counterbalance the action of the atmosphere; thence there will also arise an increase of expense, for, as I have already said, the price of steam is very high. No doubt will remain on the immense importance of this economical consideration, when I shall have stated that the Glasgow model at each oscillation expended a volume of steam several times larger than that of the cylinder. The expense of steam, or, what comes to the same thing, the expense of fuel, or, if we like it better, the pecuniary cost of keeping on the working of the machine, would be several times less if the successive heatings and coolings, the inconveniences of which have just been described, could be avoided.

This apparently insolvable problem was solved by Watt in the most simple manner. It sufficed for him to add to the former arrangement of the engine a vessel totally distinct from the cylinder, and communicating with it only by a small tube furnished with a tap. This vessel, now known as *the condenser*, is Watt's principal invention. Notwithstanding my earnest wish to abridge, I feel that I must explain its mode of action.

If there be a free communication between a cylinder full of

steam and a vessel containing neither steam nor air, the steam from the cylinder will partly and very rapidly pass into the empty vessel; the passage will only cease when the elasticity becomes equal in both. Let us suppose that by an abundant and constant injection of water, the whole capacity and the sides of the vessel be kept constantly cold, then the steam will condense as soon as it enters; all the steam which before filled the cylinder will be gradually annihilated; the cylinder will thus be cleared of steam without its sides being in the least cooled, and the fresh supply of steam, with which it will require to be filled, will not lose any of its elasticity.

The *condenser* attracts to itself all the steam contained in the cylinder, partly because it contains some cold water, and partly because it contains no elastic fluids; but as soon as some steam has been condensed, those two conditions on which success depended have disappeared; the condensing water has become hot by absorbing the latent caloric of the steam; a considerable portion of steam has been generated at the expense of that hot water; the cold water contained besides some atmospheric air which must have been disengaged during its heating. If this hot water was not carried away after each operation, together with the steam and the air contained in the condenser, in the end no effect would be produced. Watt, therefore, attains this triple purpose by the aid of a common pump, called an air-pump, and the piston of which carries a rod suitably attached to the beam worked by the engine. The power intended to keep the air-pump in motion, diminishes by that much the power of the engine; but it is only a small portion of the loss that was occasioned, in the old arrangement, by the steam being condensed on the refrigerated surface of the body of the engine.

Still another invention by Watt deserves a word, the advantages of which will become evident to everybody.

When the piston descends in Newcomen's engine, it is by the weight of the atmosphere. The atmosphere is cold, hence it must cool the sides of the metal cylinder, which is open at the top, in proportion as it expands itself over their entire surface. This cooling is not compensated during the whole ascension of the piston, without the expense of a certain quantity of steam. But there is no loss of this sort in the engines modified by Watt. The atmospheric action is totally eliminated by the following means:—

The top of the cylinder is closed by a metal cover, only pierced in the centre by a hole furnished with greased tow stuffed in hard, but through which the rod of the piston has free motion though without allowing free passage, either to air or steam.

The piston thus divides the capacity of the cylinder into two distinct and well closed areas. When it has to descend, the steam from the caldron reaches freely the upper area through a tube conveniently placed, and pushes it from top to bottom as the atmosphere did in Newcomen's engine. There is no obstacle to this motion, because whilst it is going on, only the base of the cylinder is in communication with the condenser, wherein all the steam from that lower area resumes its fluid state. As soon as the piston has quite reached the bottom, the mere turning of a tap suffices to bring the two areas of the cylinder, situated above and below the piston, into communication with each other, so that both shall be filled with steam of the same degree of elasticity, and the piston being thus equally acted upon, upwards and downwards, ascends again to the top of the cylinder, as in Newcomen's atmospheric engine, merely by the action of a slight counterpoise.

Pursuing his researches on the means of economising steam, Watt also reduced the result of the refrigeration of the external surface of the cylinder containing the piston, almost to nothing. With this view he enclosed the metal cylinder in a wooden case of larger diameter, filling the intermediate annular space with steam.*

Now the engine was complete. The improvements effected by Watt are evident; there can be no doubt of their immense utility. As a means of drainage, then, you would expect to see them substituted for Newcomen's comparatively ruinous engines. Undeceive yourselves: the author of a discovery has always to contend against those whose interest may be injured, the obstinate partisans of everything old, and finally, the envious. And these three classes united, I regret to acknowledge it, form the great majority of the public. In my calculation I even deduct those who are doubly influenced to avoid a paradoxical result. This compact mass of opponents can only be disunited and dissipated by time; yet time is insufficient, it must be attacked with spirit and unceasingly; our means of attack must be varied, imitating the chemist in this respect,—he learning from experience, that the entire solution of certain amalgams requires the successive application of several acids. Force of character and perseverance of

* It is the cylinder and piston that constitute the eminent virtue of the engine, the steam being only the *agent* employed to work the pump, so to speak. Every modification, therefore, which can promote the action of this most convenient and powerful agent is a crucial advantage. It is, therefore, that the vast improvements made by Watt—not only in working the piston-rod in the aperture of the stuffing-box, but also in promoting the uniform warmth of the cylinder by a jacket or outer casing—brought the steam-engine substantially to its present rank.—*Translator.*

will, which in the long run disintegrate the best woven intrigues, are not always found conjoined with creative genius. In case of need, Watt would be a convincing proof of this. His capital invention—his happy idea on the possibility of condensing steam in a vessel separate from the cylinder in which the mechanical action goes on—was in 1765. Two years elapsed without his scarcely making an effort to apply it on a large scale. His friends at last put him in communication with Dr. Roebuck, founder of the large works at Carron, still celebrated at the present day. The engineer and the man of projects enter into partnership; Watt cedes two-thirds of his patent to him. An engine is constructed on the new principles: it confirms all the expectations of theory; its success is complete. But in the interim, Dr. Roebuck's affairs receive various checks. Watt's invention would undoubtedly have restored them: it would have sufficed to borrow money; but our associate felt more inclined to give up his discovery and change his business. In 1767, while Smeaton was carrying on some triangulations and levellings between the two rivers of the Forth and the Clyde, forerunners of the gigantic works of which that part of Scotland was to be the theatre, we find Watt occupied with similar operations along a rival line crossing the Lomond passage. Later, he draws the plan of a canal that was to bring coals from Monkland to Glasgow, and superintends the execution of it. Several projects of a similar nature, and, among others, that of a navigable canal across the Isthmus of Crinan, which Mr. Rennie afterwards finished; some deep studies on certain improvements in the ports of Ayr, Glasgow, and Greenock; the construction of the Hamilton and Rutherglen bridges; surveys of the ground through which the celebrated Caledonian Canal was to pass, occupied our associate up to the end of 1773. Without wishing at all to diminish the merit of these enterprises, I may be permitted to say that their interest and importance was chiefly local, and to assert that neither their conception, direction, or execution required a man called James Watt.

If I were to forget my duties as the mouthpiece of the Academy, and endeavour more to make you smile than try to relate useful truths, I should find matter here for a striking contrast. I might cite this or that author who, at our weekly meetings, labours loudly to communicate some little remarks, some trifling reminiscence, some little note, conceived and got up the previous evening: I might represent him cursing his destiny, because some clause in the rules, or the order of insertion of some author, an earlier riser than himself, occasions his lecture to be deferred for a week, allowing him the guarantee, during the whole of that cruel week, of his *sealed paquet* being deposited in our archives.

On the other hand, we should see the inventor of a machine destined to form an epoch in the annals of the world, undergo without a murmur the stupid contempt of capitalists, and during eight years bend his superior genius down to surveys, plans, and minute levellings; to troublesome items of clearing or filling in, and to toises of masonry. Let us confine ourselves to supposing that Watt's philosophy led to serenity of character, moderation in desires, to true modesty. But so much indifference, however noble the cause may have been, should have its just limit. It is not without ample motive that society severely reprobates those of its members who withdraw from circulation the heaps of gold contained in their iron chests. Are we less culpable if we deprive our country, our fellow-citizens, our century, of the treasures, a thousand times more precious, resulting from the exercise of mind; if we keep to ourselves immortal inventions, sources of the most noble, of the purest enjoyments of intellect; if we do not reward the creators of mechanical combinations, which would multiply the products of industry to infinity; who would weaken, to the benefit of civilisation and humanity, the effect of the difference of position; who would some day allow the rudest manufactories to be examined without finding, in any part of them, the distressing spectacle of fathers of families and children of both sexes assimilated to brutes, advancing precipitately to their tombs?

In the early part of 1774, after contending with Watt's indifference, his friends put him into communication with Mr. Boulton, of Soho, near Birmingham; an enterprising active man, gifted with various talents.* The two friends applied to Parlia-

* In the notes which accompanied the last edition of Professor Robison's Essay on the Steam-engine, Watt expressed himself in the following terms relative to Mr. Boulton: — "The friendship that he bore me ended only with his life. The friendship that I bore him leads me to feel it my duty to avail myself of this opportunity, the last, probably, that will be offered me, to say how much I was indebted to him. It is to the earnest encouragement held out to me by Mr. Boulton, to his taste for scientific discoveries, and to the sagacity with which he applied them to the progress of the arts; it is also to the intimate knowledge he possessed of manufacturing and commercial affairs, that I attribute, in great measure, the success with which my efforts have been crowned."

Mr. Boulton had already had a manufactory for several years at Soho, when the partnership began which has rendered his name so inseparable from that of Watt. This establishment, the first that had been formed on so large a scale in England, is still quoted for its elegant architecture. Boulton used to make there all sorts of work in steel, in plated articles, in silver, in or moulu; even to astronomical clocks, and paintings on glass. During the last twenty years of his life, Boulton occupied himself with improving the coining of money. By the combination of some operations invented in France, with new presses and an ingenious application of the steam-engine, he contrived to unite an exceeding

ment for a prolongation of privilege; since Watt's Patent, dated 1769, had only a few more years to run. The bill gave rise to the most animated discussion. The celebrated mechanic wrote as follows to his aged father: — "This business could not be carried on without great expense and anxiety. Without the aid of some warm-hearted friends, we should not have succeeded, for several of the most powerful people in the House of Commons were opposed to us." It seemed to me interesting to search out to what class of society these Parliamentary persons belonged, to whom Watt alluded, and who refused to the man of genius a small portion of the riches that he was about to create. Judge of my surprise, when I found the celebrated Burke at their head! Is it possible then that men may devote themselves to deep studies, possess knowledge and probity, exercise to an eminent degree oratorical powers that move the feelings, and influence political assemblies, yet sometimes be deficient in plain common sense? Now, however, owing to the wise and important modifications introduced by Lord Brougham in the laws relative to patents, inventors will no longer have to undergo the annoyances with which Watt was teazed.

As soon as Parliament had granted a prolongation of twenty-five years to Watt's patent, he and Boulton together began the establishments at Soho, which have become the most useful school in practical mechanics for all England. The construction of draining pumps of very large dimensions was soon undertaken there, and repeated experiments showed that, with equal effect, they saved three quarters of the fuel that was consumed by Newcomen's previous engines. From that moment the new pumps were spread through all the mining counties, especially in Cornwall. Boulton and Watt received as a duty the value of one third of the coal saved by each of their engines. We may form an opinion of the commercial importance of the invention from an authentic fact: in the Chace-water Mine alone, where three pumps were at work, the proprietors found it to their advantage to buy up the inventor's rights for the annual sum of 60,000 francs (2,400*l*). Thus in one establishment alone, the substitution of

rapidity of performance with perfection of work. It was Boulton who executed for the English Government the recoining of the whole copper currency in the United Kingdom. The economy and correctness of this great work rendered counterfeiting almost impossible. The numerous executions with which the towns of London and Birmingham had been annually afflicted till then, entirely ceased. It was on this occasion that Dr. Darwin exclaimed in his *Botanical Garden*, "If at Rome a civic crown was awarded to those who had saved the life of a single fellow-citizen, did not Boulton deserve to be covered with crowns of oak by us?"

Mr. Boulton died in 1809, at the age of eighty-one years.

the *condenser* for internal injection, had occasioned an annual saving in fuel of upwards of 180,000 francs (7000*l*).*

Men are easily reconciled to paying the rent of a house, or the price of a farm. But this goodwill disappears when an *idea* is the subject treated of, whatever advantage, whatever profit, it may be the means of procuring. Ideas! are they not conceived without trouble or labour? Who can prove but that with time the same might not have occurred to everybody? In this way days, months, and years of priority would give no force to a patent!

Such opinions, which I need not here criticise, had obtained a footing from mere routine, as decided. Men of genius, the *manufacturers of ideas*, it seemed, were to remain strangers to material enjoyments; it was natural that their history should continue to resemble a legend of martyrs!

Whatever may be thought of these reflections, it is certain that the Cornwall miners paid the dues that were granted to the Soho engineers with increased repugnance from year to year. They availed themselves of the very earliest difficulties raised by plagiarists, to claim release from all obligation. The discussion was serious; it might compromise the social position of our associate: he therefore bestowed his entire attention to it, and became a lawyer. The long and expensive lawsuits that resulted therefrom, but which they finally gained, would not deserve to be now exhumed; but having recently quoted Burke as one of the adversaries to our great mechanic, it appears only a just compensation here to mention that the Roys, Mylnes, Herschels, Delucs, Ramsdens, Robisons, Murdocks, Rennies, Cummings, Mores, Southerns, eagerly presented themselves before the magistrates, to maintain the rights of persecuted genius. It may be also advisable to add, as a curious trait in the history of the human mind, that the lawyers (I shall here prudently remark that we treat only of the lawyers of a neighbouring country), to whom malignity imputes a superabundant luxury in words, reproached Watt, against whom they had leagued in great numbers, for having invented nothing but ideas. This, I may remark in passing, brought upon them before the tribunal, the following apostrophe from Mr. Rous†: "Go, gentlemen, go and rub yourselves against

* Here it must be borne in mind that a principal method of ensuring a return for their outlay, was their manufacturing steam-engines on the most extensive scale, with a degree of accuracy never before applied in the production of large machinery; and this was so fully accomplished, that all other engines were superseded. — *Translator.*

† Mr. Rous, who acted as counsel for the patentees, published his speech in the form of a pamphlet. In the text we have reproduced the English from a version of M. Arago's French, an unsatisfactory practice arising from necessity; for, in his full acquaintance with our writings, he is exuberant in quotations without always giving chapter and verse; and, moreover, many of the cited

those untangible combinations, as you are pleased to call Watt's engines — against those pretended abstract ideas; they will crush you like gnats, they will hurl you up in the air out of sight!"

The persecutions which a warm-hearted man meets with, in the quarters where strict justice would lead him to expect unanimous testimonies of gratitude, seldom fail to discourage, and to sour his disposition. Nor did Watt's good humour remain proof against such trials. Seven long years of lawsuits had excited in him such a sentiment of indignation, that it occasionally showed itself in severe expressions; thus he wrote to one of his friends:—"What I most detest in this world are plagiarists. The plagiarists! They have already cruelly assailed me; and if I had not an excellent memory, their impudent assertions would have ended by persuading me that I have made no improvement in steam-engines. The bad passions of those men to whom I have been most useful — would you believe it? have gone so far as to lead them to maintain that those improvements, instead of deserving this denomination, have been highly prejudicial to public wealth."

Watt, though greatly irritated, was not discouraged. His engines were not, in the first place, like Newcomen's, mere pumps, mere draining pumps. In a few years he transformed them into universal motive powers, and of indefinite force. His first step in this line was the invention of a *double-acting engine* (*à double effet*).

To conceive the principle of it, let my report of the *modified engine* of which I have already treated (page 545.) be consulted. The cylinder is closed; the external air has no access to it; it is steam pressure, and not atmospheric, that makes the piston descend; the ascending movement is due to a simple counterpoise, because at the moment when this takes place, the steam, being enabled to circulate freely from the higher to the lower portions of the cylinder, presses equally on the piston in both directions. Every one will hence see, that the modified engine, or Newcomen's, has power only during the descending oscillation of the piston.

A very simple change remedied this serious defect, and produced the *double-acting engine.*

In the engine known under this name, as well as in the one

passages are from letters and other manuscript documents. In the instance before us, the keen satire of Rous was in asking the opposite party whether it could be seriously contended that Watt's invention, which, during the space of nearly thirty years, had been admired in all Europe as the greatest *practical* advance ever made in the *arts*, was a mere *abstract* discovery in *science*; and, he observed, that if those who thus pleaded were to approach the *untangible substance*, as they were pleased to call it, with the same ignorance of its nature that they thus affected, they would be crushed before it like flies, leaving no trace of their existence.— *Translator.*

which we denominated the modified engine, the steam from the boiler, when the mechanic wishes it, goes freely above the piston and presses it down without meeting any obstacle; because at that same moment, the lower area of the cylinder is in communication with the condenser. This movement once achieved, and a certain cock having been opened, the steam from the caldron can enter only below the piston, and elevates it; the steam above it, which had produced the descending movement, then goes to regain its fluid state in the condenser, with which it has become, in its turn, in free communication. The contrary arrangement of the cocks replaces all things in their primitive state, as soon as the piston has regained its maximum height. Thus similar effects are reproduced indefinitely.

The motive power is here, as explained above, exclusively steam; and the engine, except by the inequality arising from the weight of the piston, has the same power whether the piston be ascending or descending. This is the reason why, from the moment of its appearance it was justly called *a double-acting engine.*

To render this new motive power of easy and convenient application, Watt had to overcome other difficulties: it was requisite to find the means of establishing a *rigid communication* between the inflexible rod of the piston oscillating in a straight line and a beam that oscillated circularly. The solution which he gave of this important problem is perhaps his most ingenious invention.

Among the constituent parts of a steam-engine, you have, no doubt, remarked a certain articulated parallelogram. With each double oscillation it developes and contracts itself, with the smoothness of motion,—I had almost said with the grace,—that charms us in the gestures of a consummate actor. Follow attentively with your eye its various transformations, and you will find it subject to the most curious geometrical conditions; that *three* of the summits of the parallelogram's angles describe arcs of circles in space, whilst the *fourth,* the summit of the angle that raises and lowers the piston-rod, moves very nearly in a straight line. The immense utility of the result strikes mechanics less than the simplicity of means by which Watt obtained it.*

* We here give Watt's words in relating the experiment of this articulated parallelogram (*this beautiful arrangement is called parallel motion — Translator*): —"I was myself surprised at the regularity of its action. When I saw it work for the first time, I felt truly all the pleasure of novelty, as if I was examining *the invention of another man.*"

Smeaton, who was a great admirer of Watt, did not believe, however, that it could in practice become a general and economical means of impressing *directly* rotatory motion to axes. He maintained that steam-engines would always be

Power is not the only element of success in industrial works. Regularity of action is not less important; but what regularity could be expected from a motive power engendered by fire fed by shovels full, and the coal itself of various qualities; and this under the direction of a workman, sometimes not very intelligent, almost always inattentive? The motive steam will be more abundant, it will flow more rapidly into the cylinder, it will make the piston work faster in proportion as the fire is more intense. Great inequalities of movement then appear to be inevitable. Watt's genius had to provide against this serious defect. The throttle-valves by which the steam issues from the boiler to enter the cylinder are not constantly open. When the working of the engine accelerates, these valves partly close; a certain volume of steam must therefore occupy a longer time in passing through them, and the acceleration ceases. The aperture of the valves, on the contrary, dilates when the motion slackens. The pieces requisite for the performance of these various changes connect the valves with the axes which the engine sets to work, by the introduction of an apparatus, the principle of which Watt discovered in the regulator of the sails of some flour-mills: this he named the *governor;* which is now called the *centrifugal regulator.* Its efficacy is such, that a few years ago, in the cotton-spinning manufactory of a renowned mechanic, Mr. Lee, there was a clock set in motion by the engine of the establishment, and it showed no great inferiority to a common spring clock.

Watt's regulator, and an intelligent use of the revolving principle, —that is the secret, the true secret, of the astonishing perfection of the industrial products of our epoch; this is what now gives to the steam-engine a rate entirely free from jerks. That is the reason why it can, with equal success, embroider muslins and forge anchors, weave the most delicate webs and communicate a rapid movement to the heavy stones of a flour-mill. This also explains how Watt had said, fearless of being reproached for exaggeration, that to prevent the comings and goings of servants, he would be served, he would have gruel brought to him, in case of illness, by tablets connected with his steam-engine. I am aware it is supposed by the generality of people, that this suavity of motion is obtained only by a loss of power; but it is an error, a gross

more serviceable in pumping water direct. This fluid having reached a suitable height, was then to be thrown into the trough, or on to the pallets of common hydraulic wheels. In this respect the prophecies of Smeaton were not realised. Yet, in 1834, on visiting the establishment of Mr. Boulton at Soho, I saw an old steam-engine still employed to raise water from a large pool, and pour it into the troughs of a great hydraulic wheel, when the season being very dry the water-power was insufficient.

error: the saying, "much noise and little work," is true, not only in the moral world, but is also an axiom in mechanics.

A few words more and we shall reach the end of our technical details.

Within these few years, great advantage has been found in not allowing a free access of steam from the boiler into the cylinder, during the *whole* time of each oscillation of the engine.* This communication is interrupted, for example, when the piston has reached one third of its course. The two remaining thirds of the cylinder's length are then traversed by virtue of the acquired velocity, and especially by the *detention* of the steam. Watt had already indicated such an arrangement.† Some very good judges esteem the economical importance of the *steam-detent* as equal to that of the condenser. It seems certain that since its adoption, the Cornwall engines give unhoped-for results; that with one bushel of coals they equal the labour of twenty men during ten hours. Let us keep in mind, that in the coal districts a bushel of coals only costs ninepence, and it will be demonstrated that over the greater part of England, Watt reduced the price of a man's day's work, a day of ten hours' labour, to *less than a sou* (*one half-penny*), of our money.‡

* This constituted Watt's celebrated expansion engine, so named because the small portion of steam already admitted, then expanded till the piston had reached the end of the cylinder.— *Translator.*

† The principle of the steam detention had been neatly expressed in a letter from Watt to Dr. Small dated 1769, it was put in practice in 1776, at Soho, and also in 1778 at the *Shadwell water works*, from economical considerations. The invention, and the advantages expected from it, are fully described in the patent of 1782.

‡ At a moment when so many people are interested in direct rotation-engines, I should be unpardonably neglectful if I did not say that Watt had both thought of them, as proved in his patents, and had made some. Watt abandoned those engines, not because they would not work, but because, in an economical point of view, they appeared to him decidedly inferior to the double-acting engines, and to those with rectilinear oscillations.

There are few inventions, large or small, amongst those of which the steam-engine offers us such an admirable assemblage, that have not been developed from some of Watt's early ideas. Follow up his labours, and besides the important points which we have minutely detailed, you will see him propose engines without condensation, engines in which the steam, after having acted, is allowed to escape into the open air, for those localities where it would be difficult to procure an abundance of cold water.

The detent intended to be used in engines having several cylinders, will also figure among the projects of the Soho engineer. He suggests the idea of perfectly water-tight pistons, though consisting entirely of pieces of metal. It was Watt also who first had recourse to mercurial gauges to appreciate the elasticity of the steam, both in the boiler and in the condenser; who imagined a simple and permanent gauge, by the aid of which, and at a glance, the height of the water in the boiler can be known; and who, to prevent this level from ever

Numerical valuations make us appreciate so well the importance of his inventions, that I cannot resist the desire to present two more improvements. I borrow them from one of the most celebrated correspondents of the Academy, from Mr. John Herschel.

The ascent of Mont Blanc, starting from the valley of Chamouni, is justly considered as the hardest work that a man can accomplish in two days. Thus, the maximum mechanical work of which we are capable, in twice twenty-four hours, is measured by transporting the weight of our body to the elevation of Mont Blanc. This work, or its equivalent, would be accomplished by a steam-engine in the course of burning one kilogram of coal. Watt has, therefore, ascertained that the daily power of a man does not exceed what is contained in half a kilogram of coal (1 lb. Avs.).

Herodotus records that the construction of the great pyramid of Egypt employed one hundred thousand men during twenty years. The pyramid consists of calcareous stone; its volume and its weight can be easily calculated; its weight has been found to be about 5,900,000 kilograms (nearly 5000 tons).

To elevate this weight to thirty-eight metres, which is the pyramid's centre of gravity, it would require to burn 8,244 hectolitres of coal (cubic metres). Our neighbours have some foundries where they consume this quantity every week.

MACHINES CONSIDERED RELATIVE TO THEIR EFFECT ON THE WELFARE OF THE WORKING CLASSES.*

Many persons, without doubting the genius of Watt, look on the inventions for which the world is indebted to him, and on the im-

varying to an inconvenient extent, connected the movements of the feeding-pump with those of a float, which, when required, was placed in an opening of the lid of the engine's principal cylinder, forming a little *indicator*, so combined as to show exactly the law of the steam's consumption relative to the piston's position, &c. If time permitted, I would show Watt not less clever, or less fortunate, in his endeavours to improve the boilers, to diminish the loss of heat, and to burn completely the torrents of black smoke that escape from common chimneys, however high they may be carried.

* In writing this chapter it seemed to me that I might unscrupulously avail myself of many documents that I had collected, either in various conversations with my friend Lord Brougham, or works that he himself has published, or that have appeared under his patronage.

If I were to attend to the criticisms that have been printed after the reading of this Biography, by trying to combat the opinion that machines are injurious to the labouring classes, I should be attacking an old prejudice that has no longer any foundation, a mere phantom. I would not ask more than to be able to believe it, for then I would very willingly suppress all my arguments, bad or good. Unfortunately some letters frequently sent me by excellent workmen, either as an academician or as a deputy; unfortunately, moreover, the recent

pulse that they have given to industrial labours, as a social misfortune. If we believed them, the adoption of each new machine inevitably adds to the troubles and miseries of labourers. Those wonderful mechanical combinations that we are accustomed to admire for the regularity and harmony of their movements, for the power and delicacy of their effects, would be instruments of injury; the legislator ought to proscribe them with a just and implacable rigour.

Conscientious opinions, especially when they are connected with praiseworthy sentiments of philanthropy, have a claim to an attentive examination. I add, that on my part this is an imperious duty. I should have neglected, indeed, the argument by which the labours of our illustrious academician are shown to be most worthy of public estimation, if, far from acceding to the prejudices of certain minds against the improvement of machines, I did not point out such works to the attention of well-meaning men, as the most powerful, the most direct, the most efficacious means of rescuing workmen from cruel sufferings, and calling them to partake of a crowd of benefits, which seemed to be regarded as the exclusive appanage of riches.

When we have to select one of two diametrically opposite propositions; when the one being true, the other must be false, and when nothing seems at first sight to be able to dictate a rational choice, geometers seize on these contrary propositions; they follow up their details carefully through all their ramifications; they make their last logical results rise up: now the ill-stated proposition, and that one only, seldom fails to lead, by wire-drawing, to some results that a clear intellect could not admit. Let us try for a moment the method of examination that Euclid often uses, and which is so justly designated by the epithet of *mode of reducing to absurdity.*

The adversaries of machines would wish to annihilate them, or at least to restrain their propagation,—to reserve, say they, more employment for the working classes. Let us, for a moment, take up this position, and the anathema will be found to extend far beyond machines properly so called.

From the beginning we shall be led, for example, to tax our ancestors with great improvidence. If instead of founding, if instead of persevering in extending the city of Paris on the two

and *ex-professo* dissertations of several economists, leave me no doubt as to the necessity of still saying, of repeating in every shape, that machines have never been the true and permanent cause of the sufferings of one of the most numerous and most interesting of the classes of society; that their destruction would aggravate the present state of things; and that it is by no means in that direction that a remedy would be found for the evils which I warmly compassionate.

banks of the Seine, they had established it in the middle of the plain of Villejuif, for centuries back the water-carriers would have formed the most occupied, the most numerous, the most important portion of the population. Well, Messrs. Economists, set to work in favour of the water-carriers. To make the Seine deviate from its course is not an impossibility; propose this undertaking; open a subscription immediately to leave Paris dry, and the general laugh will show you that the *mode of reducing to absurdity* * has some good in it, even in political economy; and in their plain sense, the workmen themselves will tell you that the river has created the immense capital in which such resources are found; and that without it, Paris would perhaps still be a Villejuif.

Good Parisians had hitherto congratulated themselves on the vicinity of those inexhaustible quarries where successive generations go to dig out materials for the construction of their temples, their palaces, their private dwellings. A mere illusion! The new political economy will prove to you that it would have been eminently advantageous if the plaster, the freestone, and rubble had been found only near Bourges for example. On this hypothesis, compute on your fingers the number of workmen that would have been required to bring to the site of the capital all the stone that during five centuries has been worked up by architects, and you will find a truly prodigious result; and however little the new ideas may smile upon you, you may go into ecstasies at your ease on the happiness that such a state of things would have shed on the proletaries.

Let us venture some doubts, although I know very well that the Vertots of our day perfectly resemble the Rhodian historian, *when their seat is made* (" *quand leur siége est fait* ").

The capital of a powerful kingdom, not very distant from France, is traversed by a majestic river, which even men-of-war ascend under full sail. The surrounding country is furrowed in all directions by canals which carry heavy burdens at a very small freightage. A regular network of routes, admirably kept up, lead to all the most distant parts of that territory. To these gifts of nature and of art, this capital, which of course every one has already named, unites *an advantage* of which Paris is deprived: the quarries of building-stone are not at its gates, they lie at a distance. There then the Utopia of the new economists is realised. Will they not now count up by hundreds of thousands, perhaps by millions, the quarrymen, the boatmen, the carters, the labourers

* This "méthode de réduction à l'absurde" is the *reductio ad absurdum*, or arguing *ex absurdo*, of logicians and mathematicians; in which the truth of a proposition is proved by showing that the contrary is unreasonable. — *Translator.*

incessantly employed, digging out, carrying away, preparing the building-stone for the construction of the immense number of edifices with which that capital is annually enriched? We will leave them to count at their ease. There has happened in that city what would have happened in Paris if it had been devoid of its rich quarries: stone being very dear, it is not used*; brick is the general substitute.

Millions of workmen are now executing, both on the surface of the earth and in its bowels, immense works which could not possibly be undertaken, if certain machines were proscribed. Two or three examples will suffice to render this truth palpable.

The carrying off the water that rises daily in the galleries of the Cornish mines alone, requires the power of 50,000 horses, or of 300,000 men. I ask you whether the pay of 300,000 workmen would not absorb all the benefit of the undertaking?

Does the question of the expense and the benefit appear to be too delicate? Other considerations will lead to the same result.

The working of one Cornish mine alone, comprised under the name of the *Consolidated Mines*, requires a steam-engine equal to upwards of 300 horses constantly in harness, and each 24 hours it realizes the work of 1000 horses. Need I fear any contradiction if I assert that there are no means of making upwards of 300 horses, or 2000 or 3000 men, labour simultaneously and to good purpose around the confined mouth of the shaft of a mine? To proscribe the steam-engine of the *Consolidated Mines* would be to reduce to inaction the great number of workmen that the engine renders it possible to employ there; it would be the same as declaring that the copper and tin of Cornwall shall remain buried there for ever, under a mass of earth, of rock, and of water several hundred meters in thickness. The thesis brought into this last form will certainly have few defenders; but what signifies the form when the substance is evidently the same?

If from labours that require an immense development of power, we were to pass on to the examination of various industrial products, which, from the delicacy of their materials and the regularity of their forms, have been placed among the wonders of art, the insufficiency, the inferiority, of our organs compared with ingenious mechanical combinations, would equally strike all minds. Where is there, for instance, so clever a spinner as to draw a thread from one pound of cotton wool fifty three leagues long, as is done by the machine called the *mule-jenny*?

I am not ignorant of what certain moralists have preached on

* This is a very incorrect expression, and might mislead a Parisian *badaud*. — *Translator*.

the inutility of muslins and laces and gossamer net, in the weaving of which this fine thread is used; but it suffices for me to remark, that the most perfect mule-jenny spins under the constant inspection of a great many workpeople; that the only requisite they care for is, to manufacture goods that will sell; in short, that if luxury is an evil, a vice, or even a crime, it is the buyers who are to blame, and not the poor proletaries, whose existence, I believe, would be very uncertain if they themselves endeavoured to manufacture for the ladies woollen stuffs instead of fashionable *tulle*.

Now let us quit remarks on details, and dive down to the very bottom of the question.

Marcus Aurelius said: "We must not receive the opinions of our fathers as children would, for the mere reason that they were our fathers' opinions." This maxim, though assuredly a very just one, ought not to prevent us from thinking, or at least from presuming, that those opinions against which no criticism has ever been pronounced from the commencement of societies, are conformable to reason and to general advantage. Well, on the question so much debated, relative to the utility of machines, what was the unanimous opinion of antiquity? Its ingenious mythology will inform us: the founders of empires, the legislators, the conquerors of tyrants who oppressed their country, received the title of *demigods* only; but it was among the gods themselves that they placed the inventors of the spade, the sickle, and the plough.

I already hear our adversaries, on account of the extreme simplicity of the instruments that I have cited, boldly refuse them the name of machines, unwilling to regard them as any thing but *tools;* and ensconce themselves obstinately behind this distinction.

I might answer that such a distinction is puerile; that it would be impossible to say precisely where the tool ends and the machine begins; but it is better worth remarking that in the pleadings against machines nothing has ever been said of their greater or less complication. If they are repudiated, it is because with their aid one man can do the work that would otherwise require several men: now would any one dare to maintain that a knife, a gimblet, a file, a saw, do not confer great facility of operation on the hand that uses them; that the hand thus strengthened would not do the work of a great many hands armed only with their nails?

The workmen, seduced by the detestable theories of some of their pretended friends, did not stop at the sophisticated distinction between tools and machines; they wandered over certain counties of England, in 1830, vociferating the cry of *down with the machines!* Rigorous logicians, they broke in the farms, the sickle intended to reap the corn, the flail that was to beat the corn, the sieve by

the aid of which the corn is winnowed. And, in fact, are not the sickle, the flail, and the sieve means for shortening labour? The spade, the hoe, the plough, the seedsower, could not find grace in the eyes of this blinded horde; and if anything surprises me, it is that in their fury they spared the horses, a sort of machine comparatively cheap to keep, and each of which could do daily as much work as six or seven men.

Political economy has fortunately obtained a place among the sciences of observation. The substitution of machines for animated beings has been so often tried during many years past, that people cannot hereafter but perceive the general results amidst some accidental irregularities. These results are as follows:

By sparing manual labour, machines enable us to manufacture at a cheaper rate; the effect of this cheapness is an increased demand, such an increased demand (so vivacious is our desire to be well off) that notwithstanding the most inconceivable lowering of price, the money value of the totality of the merchandise produced, surpasses each year what it was before the improvement; the number of workmen employed by each branch of industry increases with the introduction of means for manufacturing expeditiously.

This last result is exactly contrary to what is wished for by those who hate machines. At first sight it may appear paradoxical, yet we shall soon see it proved by a rapid examination of the most confirmed industrial facts.

When, three centuries and a half ago, the printing machine was invented, copyists used to furnish books to the very small number of rich men who could indulge in this expensive fancy. One of these copyists being able by the aid of the new proceeding to do the work of 200, there were not wanting men in that epoch who dubbed the new invention as *infernal*, as about to reduce to inaction, in a certain rank of society, 995 men out of 1000. But let us now place the real result by the side of the sinister prediction.

Manuscript books were very little in demand; printed books, on the contrary, on account of their low price, were sought after with the most lively eagerness. It was found necessary incessantly to reproduce the Greek and Roman authors. New ideas, new opinions occasioned a multitude of new books to arise; some of eternal interest, others inspired by passing events. At last it was calculated that in London, before the invention of printing, the book trade employed only 200 men, now they are counted by 20,000.

And how much more would it be if, laying aside the confined, and I might say material, point of view that I have had to select,

we were to estimate printing by its moral and intellectual phases; if we were to examine the influence that it exerted on public manners, on the diffusion of public knowledge, on the progress of human reason; if we were to work out the enumeration of the many books for which we are indebted to printing, that the copyists would certainly have disdained, and in which genius yet goes daily gathering the elements of its fruitful conceptions? But I must keep in mind that at present we have only to treat of the number of workmen employed by each branch of industry.

That of cotton offers even more demonstrative results than is done by printing. When Arkwright, an ingenious barber of Preston, (who, by the way, left each of his children two or three millions of francs of income,) rendered it both useful and profitable to substitute revolving cylinders for the fingers of the women who used to spin, the annual product of the cotton manufacture in England did not exceed 50,000,000 francs (2,000,000*l.*), now it exceeds 900,000,000 francs (36,000,000*l.*). In the county of Lancaster alone, they annually deliver to the calico manufacturers a quantity of yarn that 21,000,000 clever spinners could not accomplish with only the aid of the rock and spindle. Moreover, although in the art of spinning mechanical means have been pushed, we may say, to their utmost degree of perfection, 1,500,000 people now find occupation there, where, before the inventions of Arkwright and of Watt, there were only 50,000.*

A certain philosopher exclaimed, in a deep fit of despondency, "Nothing new is published in the present day, unless we call new that which has been forgotten." If the philosopher alluded only to errors and prejudices, he spoke truth. Time has been so fruitful in this line, that no one can any more claim priority. For example, the pretended modern philanthropists have not the merit (if there be any merit in it) of inventing the systems that I am examining. Rather look at that poor William Lea (*Lee*), working the first stocking frame in the presence of James I. The mechanism appeared admirable; why was he repulsed? It was under the pretext that the working class would suffer. France showed herself equally short-sighted: William Lea found no encouragement there, and he was reduced to die in a hospital; like so many other men of genius, who have had the misfortune of being too much a-head of their age!

* Mr. Edward Baines, author of a much esteemed work on the British cotton manufactures, has had the whimsical curiosity to learn what length of thread is annually used in weaving the cotton manufactures. ' This entire length he finds to be equal to *fifty-one times the distance of the sun from the earth!* (fifty-one times thirty-nine millions of post leagues, or about two thousand millions of such leagues.)

Besides this, we should be very much mistaken if we supposed the body of knitters very numerous, to whom William Lea fell a victim. In 1583, it was only people of high rank and fortune who wore stockings. The middle class substituted for this portion of modern dress narrow stripes of variously coloured cloths. The rest of the population (nine hundred and ninety-nine out of a thousand) walked bare-legged. In consequence of the extremely low price of stockings in the present day, there is not above one man in a thousand who cannot afford to buy them. Hence an immense number of workmen, in every part of the world, is now employed in this branch of manufacture.*

If it be deemed necessary I will add, that at Stockport, the substitution of steam power for manual labour in weaving looms, has not prevented the workmen from increasing by one third in a few years.

We must now deprive our adversaries of their last resource; we must not leave them the power of saying that we have only cited *old* branches of industry. I will, therefore, now remark how much they were, not long since, deceived in their lugubrious forebodings relative to engraving on steel. A copper plate, they said, will not give above two thousand impressions. A steel plate, by yielding a hundred thousand without being worn, would replace fifty copper plates. Will not these numbers prove that the greater part of the former engravers (forty-nine out of fifty) will feel obliged to abandon their profession, to change their graver for the trowel or the hoe, or beg charity in the public streets?

For the twentieth time, prophets of evil, be pleased not to forget in your lucubrations, the principal element of the problem which you undertake to solve! Think of the insatiable desire to be well off, that Nature has implanted in the human heart; remember that one wish is no sooner satisfied, than it immediately gives rise to another wish; that our appetites of every sort increase with the cheapness of the objects adapted for their indulgence, and to a degree that defies the creative powers of the most powerful machines.

* This is certainly an epochal point of great interest in domestic manufacture. The bandaged stocking is of a very remote date, and is found in all the Saxon figures of our missals and monuments; it was in common use among the peasantry of Europe even during the fifteenth century. Henry VIII., it appears, wore silk stockings, and Queen Elizabeth refused to wear any others, whence they came into vogue. These seem to have been brought from abroad; but in 1564, William Rider, an apprentice on London Bridge, borrowed a pair of knit worsted stockings from Mantua, took the hint, and made a pair exactly like them, which he presented to the Earl of Pembroke. And these are the first pair of worsted stockings known to be knit in England: the prototype of millions upon millions. — *Translator.*

But to return to the engravings. An immense majority of the public did without them when they were dear; their price decreases, and all the world seeks for them. They have become the necessary ornament of the best books; to middling books they give some chance of sale. There are no almanacks even now, but what the old hideous figures of Nostradamus, by Matthew Laensberg, are replaced by picturesque views which, in a few seconds, transport our immoveable citizens from the shores of the Ganges to those of the Amazon, from the Himalayas to the Cordilleras, from Pekin to New York. Look also at those engravers, whose ruin was so piteously announced to us; never were they either more numerous or more occupied.

I am going to bring forward some irrefragable facts. They will render it impossible, I think, to maintain that among the inhabitants of this earth, such, at least, as Nature has created, the use of machines can bring on the result of a diminution of the number of workmen employed in each sort of industry. Other customs, other habits, other passions, might, perhaps, have led to an entirely different result; but I leave such a text to those who may be tempted to write treatises on political economy for the use of the inhabitants of the Moon, or of Jupiter, or of Saturn.

Placed in a much more confined theatre, I ask myself whether, after having sapped the very foundations of the system maintained by the adversaries of machines, it can be still requisite to cast a glance on some criticisms of detail. Need we remark, for example, that the poor's-rate, that bleeding wound in the British nation, that wound which some people pretend to trace to the abuse of machines, dates from the reign of Elizabeth, from a period anterior by two centuries to the labours of Arkwright and of Watt?

You will at least acknowledge, they say to us, that the fire-machines, the mule-jennies, that the machines used for carding, for printing, &c., objects of your predilection, have not prevented pauperism from increasing and propagating itself. This fresh avowal will cost me but little. Did any one recommend machines as a universal panacea? Was it ever maintained that they would have the unheard-of property of discarding error and passion from political assemblies? that they would direct the counsellors of princes to the paths of wisdom, of moderation, of humanity? Was it ever pretended that they would turn Pitt from unceasingly meddling in the affairs of neighbouring countries? from annually raising enemies to France in every part of Europe?* from paying

* It is to be regretted that our author should allow his prejudices, which we happen personally to know were very strong, thus again to run him off the rail, and forget his promise to enter upon "*ce sujet avec la ferme volonté d'être im-*

them large subsidies, in short, from loading England with a debt of many hundred millions? There, there is the reason why the poor's-rate has increased so fast, and so prodigiously. Machines have not, and could not, produce this evil. I dare even assert that they much diminish it; and I will prove it in a few words. The county of Lancashire carries on more manufactures than any other in England. It is there that we find the cities of Manchester, Preston, Bolton, Warrington, and Liverpool; it is in that county that machines were most quickly and most generally introduced. Well, let us distribute the entire annual amount of the poor-rates of Lancashire among the whole of its population; in other words, let us learn what would be the quotient for each individual; and we shall find a result nearly three times smaller than the mean of all the other counties! You see, numbers are pitiless towards the inventors of theories.

Moreover, let not these large words of poor-rates make us believe, on the faith of some declaimers, that the labouring classes among our neighbours are entirely devoid of resources and foresight. A work of recent date has shown, that in England alone (Ireland and Scotland being thus left aside), the capital belonging to mere workmen, that has been deposited in the savings' banks, amounts to nearly four hundred millions of francs (16,000,000*l.*). The verification of property in all the principal towns is not less instructive.

One principle only has remained uncontested, amidst all the animated debates to which political economy has given rise: and this is, that population increases with general comfort, and that it diminishes rapidly in times of scarcity.* Let us place facts by the side of the principle. Whilst the mean population of England was increasing during the last thirty years 50 per cent., Nottingham and Birmingham, two of the most industrial cities, presented a still larger augmentation, to the amount of 25 and even 40 per cent.† Finally, Manchester and Glasgow, which occupy the first rank in the whole British empire, from the number, the size, and

partial." While he assails "Pitt," he contrives to omit the provocation and necessity of the case: but assuredly it was not Pitt who broke loose, and among other wild and unprecedented phantasies, invited all the nations of Europe to demolish their governments. Yet what has all this to do with James Watt? — *Translator.*

* Ireland is an exception to this rule, the cause of which is well known; and I shall have occasion to recur to it.

† This sentence would have been clearer, had the author, instead of *accroissements de 25 et de 40 pour 100 plus considérables encore,*" said 75 and even 90 per cent.; because he means 25 added to the above 50, and 40 added to the 50 per cent., — since he gives them as instances of larger augmentations. — *Translator.*

the importance of the machines that they employ, saw their population, in the same period of the last thirty years, increase from 150 to 160 per cent. This was three or four times more than in the agricultural counties and towns not possessing manufactories.

Such numbers speak for themselves. No sophistry, no false philanthropy, no efforts of eloquence, can resist them.

Machines have given rise to a particular kind of objection, which I must not pass over in silence. At the time of their introduction, at the time when they begin to take the place of manual labour, certain classes of workmen suffer from the change. Their honourable, their laborious industry, is almost suddenly annihilated. Even those who under the old system were the cleverest, being sometimes devoid of the qualities required in the new operations, remain unemployed. They seldom succeed all at once in adapting themselves to a new kind of labour.

These reflections are just and true. I will add that the bad consequences pointed out by them must often recur: some caprice in the fashions even, suffice to produce deep misery. If I do not conclude from thence that the world ought to remain stationary, God forbid that in wishing the general interests of society to advance, I would have it remain deaf to the individual sufferings caused by this temporary advance! Authority, always on the watch relative to new inventions, seldom fails to catch them by fiscal measures. Would it be expecting too much from it, were it asked to allow the first contributions levied on genius to be devoted to opening some special workshops where the artisans who had been suddenly dispossessed of their occupation should, for a time, find employment consonant to their powers and their knowledge? This method has sometimes been found successful; it would then only remain to generalise it. Humanity makes it a duty, and sound policy counsels it; besides which, some terrible events, of which history preserves the recollection, would also recommend this method on the part of economy.

The objections of theorists who feared that the progress of mechanics would reduce the working class to complete inaction, have been followed by others quite of a different character, and on which it appears indispensable to dwell for a moment.

By suppressing all efforts of strength in manufactories, machines allow of a great number of children of both sexes being called in. Some industrial and some avaricious parents often abuse this power. The time devoted to work is most unreasonably long. For the daily pay of eight or ten centimes, some minds are devoted to eternal stupidity, which a few hours of study would have rendered fruitful; limbs are condemned to be rickety, that required

open air, and exercise in the wholesome rays of the sun for their development.

To ask a legislator to put an end to this hideous exhaustion of the poor by the rich; to solicit measures for combating the demoralization which is the usual consequence of numerous gatherings of young work-people; to try to introduce and distribute certain machines among the cottages, so that, according to the seasons, agricultural labours might be interspersed with the industrial; — this would be an act of humanity, an act of patriotism; this would show a knowledge of the present requirements of the working classes. But to persist in executing laboriously and expensively, by hand, works that machines can perform in a twinkling and at a low price, would but assimilate proletaries to brutes. To ask daily efforts from them which ruin their health, and which science can procure a hundredfold by the action of wind, of water, or of steam, would be going in the contrary direction to the wished-for result: this would be devoting the poor to nudity; reserving exclusively for the rich a host of enjoyments, which at present are shared by everybody: this, in short, would be, from sheer inconsiderateness, going back to the age of ignorance, to barbarity, and to misery.

It is time to quit this subject, although I am far from having exhausted it. I shall not assuredly have triumphed over a crowd of inveterate and systematic prejudices: but I may hope, at least, that my pleading will obtain the concurrence of those thousands of idlers in the capital, whose life is passed in proportioning a taste for pleasures with their bad health. In a few years, thanks to Watt's discoveries, all these Sybarites, incessantly impelled by steam along railways, can rapidly visit the various regions of the kingdom. They can go the same day to see the fleet get under weigh at Toulon; breakfast at Marseilles on the succulent roach of the Mediterranean; at noon plunge their enervated limbs in the mineral waters of Bagnères; and return at night, by way of Bordeaux, to the ball or the opera! Do you doubt this? I shall say that my itinerary only supposes a rate of twenty-six leagues per hour; that several trials of steam carriages have realised a velocity of fifteen leagues; that Mr. Stephenson, in short, the celebrated engineer of Newcastle, offers to construct steam-engines two and a half times more rapid: engines which will accomplish forty leagues per hour!

PRESS FOR COPYING LETTERS. — HEATING BY STEAM. — COMPOSITION OF WATER. — BLEACHING BY THE AID OF CHLORINE. — ESSAY ON THE PHYSIOLOGICAL EFFECTS THAT MAY RESULT FROM BREATHING VARIOUS GASES.

When Watt went to reside at Soho, Birmingham counted Priestley among the inhabitants of its vicinity,— Priestley, and his name alone says all; Darwin, the author of the *Zoonomia*, and of a celebrated poem entitled *The Loves of the Plants;* Withering, a distinguished physician and botanist; Keir, a chemist well known by his notes to his translation of Macquer, and by an interesting memoir on the crystallization of glass; Galton, author of an elementary treatise on Ornithology; Edgeworth, author of various works justly appreciated, and father of the so celebrated Maria. These learned men soon became friends of the illustrious mechanic, and most of them formed, with him and Boulton, an association called the *Lunar Society*. Such a whimsical appellation gave rise to many mistakes: it only meant that they met on the evening of full moon, a time of the month chosen by preference, in order that the members might see their way home.

Each sitting of the Lunar Society was, for Watt, a fresh opportunity for showing the remarkable fecundity of invention with which Nature had endowed him. Darwin said one day to his companions, "I have imagined a certain double pen, a pen with two beaks, by the aid of which we may write everything in duplicate; and thus at once give the original and the copy of a letter." Watt almost immediately replied: "I hope to find a better solution of the problem. I will work out my ideas to-night, and will communicate them to you to-morrow." The next day the copying press was invented, and even a small model allowed already of an opinion being formed of its effects. This instrument, so useful and so generally adopted in all the English counting-houses, has recently received some modifications, an honour claimed by many workmen; but I can assert that the present form was actually described and drawn in 1780, in the patent of our associate.

Warming by steam was more recent by three years. Watt adopted it in his own house in 1783. We must acknowledge that this ingenious method is found indicated in the *Philosophical Transactions* for the year 1745 by Colonel Cooke *; but the idea

* I read in a work by Mr. Robert Stuart that Sir Hugh Platte had an idea, before Colonel Cooke, of the possibility of applying steam to warming dwellings. In the *Garden of Eden* by that author, published in 1660, something is said to that effect for preserving plants through the winter in the green-house. Sir Hugh Platte proposes placing lids, made of tin or of any other metal, over the

passed away unheeded. At all events, Watt will not have the honour only of reviving it: he was the first to apply steam; it was his calculations on the extent of surface requisite for the warming of halls of various sizes, that in the beginning served as a guide to the greater part of the English engineers.

If Watt had only produced, in the course of his long life, the separate condenser for the steam-engine, the detent for regulating the steam, and the articulated parallelogram, he would still occupy one of the highest places among the small number of men whose life marks an epoch in the history of the world; but his name seems to me to be splendidly connected with the greatest and the most important discovery in modern chemistry: the discovery of *the components of water*. My assertion may be daring, for the numerous works in which this essential point of the history of the sciences is treated *ex professo,* have forgotten Watt. I hope, however, that you will follow my discussion without prejudice; that you will not allow yourselves to be deterred from the examination, by some authorities which are not however so numerous as might be supposed; that you will not refuse to remark how few authors in the present day refer back to original sources; how disagreeable they find it to disturb the dust of libraries; and, on the contrary, how convenient to feed on the erudition of other people, to reduce the composition of a book to the mere work of editing. But the promise that I hold of your confidence in me, has appeared of more weight. I have pressed into my service a number of printed memoirs, the whole of a voluminous authentic correspondence still in manuscript; and if after fifty years I come to claim for Watt an honour that has been inconsiderately granted to one of his most illustrious countrymen, it is because it has seemed to me beneficial to show that in the heart of academies, truth is sure to shine out sooner or later, and that in matters of discovery, there is never any prescription.

The theory of the four pretended elements, fire, air, water, and earth, the varied combinations of which were to produce all known bodies, is one of the numerous legacies that have come down to us from the brilliant philosophy which, through several centuries, dazzled and misled some of the noblest intellects. Van Helmont was the first who shook, though slightly, one of the principles of this ancient theorem, by pointing out to the attention of chemists, various permanently elastic fluids, several sorts of air, that he called *gases,* the properties of which differed from those of common air, from those of the elemental air. Boyle's and Hooke's expe-

saucepans in which the viands are being cooked, and then to certain openings in these lids to adapt tubes, by which the heating steam may be led wherever it is desired.

riments raised more serious difficulties still: they prove that common air, necessary to respiration and to combustion, undergoes remarkable changes in the course of those two phenomena, and exhibits changes of properties which imply the notion of composition. The numerous observations by Hales; the successive discoveries of carbonic acid by Black, of hydrogen by Cavendish; of nitrous acid, of oxygen, of muriatic acid, of sulphurous acid, and of ammonia by Priestley, definitively banished the old idea of there being a unique and elementary air; that being among the almost constantly false conceptions hazarded by people, who have the audacity to think themselves called upon, not to discover, but to guess the course of nature.

Amidst so many remarkable incidents, water had still preserved its character as an element. The year 1776 * was at last signalised by one of those observations that were to lead to the upsetting of this general belief. It must be acknowledged that we must also assign the same year for the singular efforts made by the chemists, not to surrender to the natural results of their experiments. The observation of which I wish to speak belongs to Macquer.

That judicious chemist having placed a white porcelain saucer over the flame of hydrogen gas which was burning tranquilly out of the mouth of a bottle, remarked that this flame was not accompanied by any smoke properly so called; that it deposited no soot; that the part of the saucer which was *licked* by the flame was, on the contrary, evidently covered by small drops of a fluid resembling water, and which, after verification, was found to be pure water. This was certainly a singular result. Observe carefully, it was in the midst of the flame, on that part of the saucer which was *licked* by it, as Macquer says, that the little drops of water were deposited. The chemist, however, did not dwell upon this fact; he was not surprised at what it contains of surprising; he simply cites it without any commentary; he does not perceive that he was touching a great discovery with his finger.

Should genius then, in the sciences of observation, be reduced to the faculty of asking, at appropriate times, *why?*

The physical world enrols volcanoes that have never made but one eruption. It is the same in the intellectual world; for there are men who, after a flash of genius, entirely disappear from the history of science. Such was Warltire, whom I am here led to cite by the chronological order of dates for a truly remarkable

* It was in this year, 1776, according to Priestley, that Volta fired inflammable air by the electric spark: the experiment of Macquer appears to have been made two years afterwards. — *Translator.*

experiment. At the commencement of the year 1781, this physicist imagined that an electric spark could not pass through certain gaseous mixtures without occasioning some decided changes in them. So novel an idea, unsuggested by any previous analogy, but of which such happy applications have since been made, would have merited for its author, I think, some honourable mention on the part of the historians of science. Warltire was wrong as to the changes that electricity would create, but fortunately for him he did foresee that an explosion would accompany them. It was therefore that he made the experiment in a metallic vase, having enclosed some air and some hydrogen in it.

Cavendish soon repeated Warltire's experiment. The *positive date* of his repetition (I call thus all dates resulting from an authentic deposit, or an academical lecture, or a printed paper) is anterior to the month of April 1783, since Priestley cites Cavendish's observations in a memoir of the 21st of that month. The citation besides informs us only of one circumstance; it is, that Cavendish had obtained *water* by the detonation of a mixture of oxygen and hydrogen, a result already proved by Warltire.

In his April memoir, Priestley added a remarkable circumstance to those which resulted from the experiments of his predecessors, for he proved that the weight of the water deposited on the sides of the vase at the moment of the oxygen's and hydrogen's detonation, is the sum of the weight of both the gases.

Watt, to whom Priestley communicated this important result, immediately saw in it, with the penetration of a superior mind, that water is not a simple body.

He therefore wrote to his illustrious friend: "What are the products of your experiments, *water*, *light*, and *heat?* Are we not then authorised from hence to conclude that water is a union of oxygen and hydrogen gas, deprived of a portion of their latent or elementary heat; that oxygen is water deprived of its hydrogen but united to latent heat and light?"

"If light be only a modification of heat, or only a circumstance attendant on its manifestation, or a component part of hydrogen, oxygen gas must be water deprived of its hydrogen, but united to some latent heat."

This passage, so clear, so neat, so methodical, is extracted from a letter by Watt, of the 26th of April, 1783. The letter was communicated by Priestley to several learned men in London, and referred immediately after to Sir Joseph Banks, President of the Royal Society, to be read at a meeting of that learned society. Some circumstances, which I suppress because they are irrelevant to our present purpose, retarded the reading by a year; but the

letter remained in the archives of the Society.* It is inserted in the seventy-fourth volume of the *Philosophical Transactions,* under its true date of the 26th of April, 1783. It is found there inserted in a letter from Watt to De Luc, dated 26th of November, 1783, distinguished by inverted commas, applied by the Secretary of the Royal Society.

I do not ask for indulgence on this profusion of details, it will be perceived that a minute comparison of dates could alone bring the whole truth to light; and that the subject is one of those discoveries that do most honour to the human mind.

Among the pretenders to this fruitful discovery, we are now going to see arise the two greatest chemists boasted of by France and England. Everybody must have already named to themselves Lavoisier and Cavendish.

The date of the public reading of the memoir in which Lavoisier detailed his experiments, in which he developed his views on the production of water by the combustion of oxygen and hydrogen, is posterior by two months to Watt's letter (already analysed) being deposited in the archives of the Royal Society of London.

The celebrated memoir by Cavendish, entitled *Experiments on Air,* is more recent still; it was read the 15th of January, 1784. It might excite reasonable surprise that facts so well authenticated should have become the subject of such an earnest polemical dispute, if I did not hasten to lay before you a circumstance that I have not mentioned before. Lavoisier declared, in positive terms, that Blagden, Secretary of the Royal Society of London, was present at his first experiments on the 24th of June, 1783, and that "he informed him that Cavendish, having already tried in London to burn hydrogen gas in closed vessels, had obtained a very sensible quantity of water."

Cavendish also repeated in his memoir, the communication made by Blagden to Lavoisier. According to him, it was more detailed than the French chemist had acknowledged. He said, that the information included the conclusion to which the experiments led, that is to say, the theory of water being a compound.

Blagden, being called to account, wrote in the Journal of Creil, in 1786, to confirm the assertion made by Cavendish.

If we believe this, the experiments of the Academician of

* To this diffident and philosophical document we refer the reader; in it Watt states that he feels great reluctance to lay his thoughts "before the public in their present indigested state, and without having been able to bring them to the test of such experiments as would confirm or refute them." M. Arago, in rendering portions of the paper, resorts to the exact chemical language of the present day; whence he uses *hydrogène* for inflammable air and phlogiston, and *oxygène* for dephlogisticated air. — *Translator.*

Paris would not have been more than a simple verification of those made by the English chemist. He assures us that he announced to Lavoisier, that the water obtained in London was precisely equal in weight to the sum of the weight of the two gases that had been burned. And Blagden finally adds: "*Lavoisier told the truth, but not all the truth.*"

Such a reproach is severe; but if it were well founded, should I not diminish its weight very much, if I were to show that excepting Watt, all those whose names figure in this story more or less exposed themselves?

Priestley details some experiments as if they were his own, and it results from them that the water engendered by the detonation of a mixture of oxygen and hydrogen, weighs exactly the same as the two gases burnt. Some time after, Cavendish claims this result as his own, and insinuates that he had communicated it verbally to the Birmingham chemist.

Cavendish infers from this similarity of weight, that water is not a simple body; yet he makes no mention of a memoir deposited in the archives of the Royal Society, in which Watt developed the same theory. It is true, that at the day of publication the name of Watt is not forgotten; but it was not in the archives that the celebrated engineer's labours could be seen: they are declared to have become known, by a recent reading, at the public meeting. At the present day, however, it is perfectly agreed that this reading followed, by several months *, that of the memoir in which Cavendish alludes to it.

On reaching the field of this serious discussion, Blagden announces his firm intention to elucidate everything, to correct everything. And in fact he did not draw back from any accusation, from any inquiry into dates, as long as the object was to insure to his patron and friend, Cavendish, a priority above the French chemist. But as soon as his explanations concerned two of his countrymen, they became vague and obscure. He says:— "In the spring of 1783, Mr. Cavendish showed us that he was led to conclude from his experiments, that water is nothing but oxygen deprived of its phlogiston (that is to say, deprived of its hydrogen). *About the same time,* the news reached London, that Mr. Watt of Birmingham had been led by some observations to a similar result." This expression, *about the same time,* to speak in Mr. Blagden's own style, would not be *all the truth. About the same time* decides nothing: questions of priority might depend on weeks, on days, on hours, on minutes. To be clear and precise, as had been promised, he should have said whether the verbal

* This delay, it should be observed, was in consequence of Watt's own request; his reason for so doing is shown in the note to page 571.— *Translator.*

communication made by Cavendish, to several members of the Royal Society, preceded or followed the arrival in London of the news of Watt's experiments. Can it be supposed that Blagden would not have explained himself on a fact of this importance, if he could have quoted an authentic date in favour of his friend?

To render the complication complete, the correctors of the press, the compositors, the printers, of the *Philosophical Transactions*, all took part in this affair. Several dates are incorrectly given. On the separate copies of his memoir which Cavendish distributed among various learned men, I perceive an error of a whole year.* By a sad fatality, for it is a real misfortune unwillingly to give rise to painful and undeserved suspicions, not one of these numerous errors of the type was favourable to Watt! God forbid that I should mean, by these remarks, to criminate the literary probity of the illustrious philosophers whose names I have cited: they only prove that in matters of discovery, strict justice is all that ought to be expected from a rival or competitor, however eminent his reputation may already be. Cavendish could scarcely listen to people on business, when they went to consult him about the investment of his twenty-five or thirty millions (*a million sterling or more*); but you now know whether he felt equally indifferent about experiments. It would not be requiring too much, then, if the historians of science were not to receive, as available titles to property, any but written titles; perhaps, I ought rather to say, any but published titles. Then, and only then, would those quarrels end, which are continually recommencing, by which national vanity generally suffers; then the name of Watt would resume in the history of chemistry the high post that is his due.

When the solution of a question of priority, like the one we have been discussing, is founded on the most attentive examination of printed memoirs, and on a minute comparison of dates, it assumes the character of a real demonstration. Still I feel myself bound to give a rapid glance at the various difficulties to which

* Our author must have been excited here, for he thinks that not only the high-minded Cavendish and Blagden, but even the printers of the papers, were in a conspiracy against Watt; and, though he calls God to witness that he means nothing against their probity, he makes a very bold insinuation that they were leagued against truth. The separate copies of Cavendish's paper, pulled off for private distribution, were dated 1783 instead of 1784; as soon as the error was discovered, means were taken to correct it. Such an accidental error occurs in Watt's own communication in the seventy-fourth volume of *Transactions*; it being there said to have been read in April 1784, though stated to have been written in November 1784.— *Translator.*

some very good intellects appear to me to have attached importance.

How can it be admitted, I have been asked, that in the midst of an immense whirlpool of commercial affairs, that pre-occupied by a multitude of lawsuits, that obliged to provide by daily inventions against the difficulties of a rising manufactory, Watt could find the time to follow the progress of chemistry step by step, to make fresh experiments, to propose explanations which the masters of the science themselves would not have thought of?

To this difficulty I shall make a short but conclusive reply: I hold in my hands the copy of an active correspondence principally relative to chemical topics, that Watt kept up during the years 1782, 1783, and 1784, with Priestley, Black, De Luc, the engineer Smeaton, Gilbert Hamilton (of Glasgow), and Fry (of Bristol).

The following is an objection that appears more specious; it arises from a deep knowledge of the human heart.

The discovery of the composition of water, advancing step by step with the admirable inventions that are united in the steam-engine, can we suppose that Watt would, from inconsiderateness, or at least without showing any displeasure, allow himself to be despoiled of the honour which it would eternally shed on his name?

This reasoning has the defect of erring completely at its very basis. Watt never yielded the share that legitimately belonged to him in the discovery of the composition of water. He had his memoir carefully printed in the *Philosophical Transactions.* A detailed note authentically proved the date of the presentation of the various paragraphs of that writing. What could, what ought, a philosopher of Watt's character to do, otherwise than patiently await the day of retribution? However, a piece of awkwardness on the part of De Luc had well nigh dragged our associate from his forbearance. The Genevese physician, after having warned the illustrious engineer of the inexplicable absence of his name from the first edition of Cavendish's Memoir, and after having described this omission in terms which the high renown of both parties does not allow me to repeat, writes to his friend: "I should almost advise you, considering your position, to extract practical results from your discoveries, for the sake of your purse."

These words offended Watt's high mind; he replied: "If I do not immediately claim my rights, you must impute it to an indolence of disposition, which leads me to feel it easier to bear with injustice, than to struggle for redress. And as to considerations of pecuniary interest, they are of no value in my estimation. Besides which, my future depends on the encouragement that the

public may be inclined to grant me, and not at all on that of Mr. Cavendish or of his friends."

Ought I to fear, that I have attached too much importance to the theory that Watt imagined for explaining Priestley's experiments? I think not. Those who would refuse a rational consent to this theory, because it now seems the inevitable result of facts, must forget that the finest discoveries achieved by the human mind have been, above all, remarkable for their simplicity. What did Newton* himself do, when, repeating an experiment that had been known already for fifteen centuries, he discovered white light? He gave so natural an interpretation of this experiment, that it now seems impossible to offer any other; he says—"All that is obtained, by any proceeding whatever, from a pencil of white light, must have been contained in it in a state of mixture. The glass prism possesses no creative property. If the parallel ray, infinitely diluted with the solar light that falls on the first surface, issues from the second surface, diverging and with a sensible breadth, it is because the glass separates that which, in the white pencil, was by its nature unequally refrangible." These words are nothing but a literal translation of the known experiment of the prismatic solar spectrum. This translation, however, had escaped an Aristotle, a Descartes, a Robert Hooke.

Without departing from the subject, let us come to some arguments which will lead in a still more direct line to the point. The theory conceived by Watt of the composition of water reaches London. If it had been according to the ideas of those times, as simple, as self-evident as it appears to us now, the counsel of the Royal Society would not have failed to adopt it: but its strangeness made them doubt the correctness of Priestley's experiments. They went so far as to laugh at it, said De Luc, *as at the explanation of the golden tooth.*

A theory, the conception of which did not present any difficulty, would certainly have been despised by Cavendish. But recollect

* This is barely in point; some of the phenomena of colours were certainly known before the advent of Newton, but that *princeps philosophorum* formed the prismatic spectrum itself, by which the spaces occupied by the successive colours were accurately defined, the colours submitted to a similar analysis, and the white light re-formed; thus ascertaining and proving that light, instead of being homogeneous as had been supposed, was actually a heterogeneous mixture of differently refrangible rays. Nor do we quite quadrate with the lengthy discussion before us, since we do not consider the case—*in rê* the beautiful composition of water—to be conclusively established. To those readers who are interested in so crucial a point in scientific history, we recommend a perusal of Vernon Harcourt's remarkable address to the British Association at Birmingham, in 1839; it being alike free from reckless assertion, and that hot nationality which warps judgment.—*Translator.*

with what eagerness, under the inspiration of that man of genius, Blagden claimed the priority of it against Lavoisier.

Priestley, to whom a great part of the honour of Watt's discovery belonged,— Priestley, whose affectionate sentiments for the celebrated engineer cannot be doubted, wrote to him, under date of the 29th April, 1783,—" Look with surprise and indignation at the drawing of an apparatus, by the aid of which I have *irrevocably undermined your beautiful hypothesis*."

In conclusion, a hypothesis at which they laughed in the Royal Society; which made Cavendish emerge from his habitual reserve; which Priestley, laying all self-love aside, endeavoured to upset, deserves to be registered in the history of science as a great discovery, whatever opinion we may entertain of it at the present day, owing to knowledge that has become common.*

The art of bleaching by means of chlorine, that beautiful invention of Berthollet's, was introduced into England by James Watt, after the journey he made to Paris about the end of 1786. He constructed all the requisite apparatus, directed its establishment, presided at the first trials; and then confided to Mr. Macgregor, his father-in-law, the management of the new art. Notwithstanding the solicitations of the illustrious engineer, our celebrated countryman *had obstinately refused* † to become a partner in an enterprise which offered no unfavourable chance, and the profits of which, it seemed, must become very great.

Scarcely had they discovered, during the second half of the last century, the numerous gaseous substances, which now act so important a part in the explanation of chemical phenomena, when they thought of rendering them useful in medicine. Dr. Beddoes followed up this idea with sagacity and perseverance. Private subscriptions even enabled him to establish a *Pneumatic Institution* at Clifton, near Bristol, where the therapeutic properties of all the gases were to be carefully studied. The Pneumatic Institution had for some time the advantage of being under the direction of the young Humphry Davy, who was then entering on his scientific career. It could boast also of having James Watt as

* Lord Brougham was present at the public meeting, where, in the name of the Academy of Sciences, I paid this tribute of gratitude and admiration to Watt's memory. On his return to England he collected some valuable documents, and again studied the historical question to which I have devoted so much space, with the superiority of perception which is familiar to him, with the scrupulousness, in some measure justiciary, which might be expected from a former Lord Chancellor of Great Britain. I owe to a degree of kindness, of which I feel the full value, the advantage of being able to offer the public the hitherto unpublished researches of my illustrious co-academician. See the end of this *éloge*.

† This expression is correct, however fabulous it may now appear.

one of its founders. The celebrated engineer did more: he imagined, described and executed, in his manufactory at Soho, the apparatus which generated the gases; and he administered it to the patients. I have found several editions of his Memoir treating of these researches* under the several dates of 1794, 1795, 1796.

Our associate's attention was attracted to this subject, in consequence of his being cruelly deprived of several friends and relations before the usual age, by diseases of the chest. It was chiefly the *lésion* of the respiratory organs that Watt thought might be treated by the aid of the specific properties of the new gases. He also expected some advantage from the action of the impalpable molecules, of iron, and of zinc, which hydrogen carries along with it when prepared in a certain way. I will finally add, that among the numerous medical notes published by Dr. Beddoes, and announcing results more or less decisive, there is one signed John Carmichael, relative to the radical cure of hæmoptysis in a servant, Richard Newberry, who was made at certain times to breathe a mixture of steam and carbonic acid by Watt himself. Although I am quite aware of my utter incompetence on such a subject, may I not be permitted to regret that a treatment which counted a Watt and a Jenner among its adherents, has been entirely abandoned, although no series of experiments can be cited in manifest opposition to those of the Pneumatic Institution at Clifton.†

WATT IN PRIVATE LIFE.—DETAILS OF EVENTS AND OF HIS DISPOSITION. —HIS DEATH.—NUMEROUS STATUES ERECTED TO HIS MEMORY.—REFLECTIONS.

Watt had married, in 1764, his cousin Miss Miller. She was an accomplished person, of superior mind, and whose never failing sweetness and cheerfulness of disposition soon raised the celebrated engineer from the indolence, the melancholy and the misanthropy that a nervous illness and the injustice of man threatened to render fatal. But for Miss Miller, Watt would probably never have made his beautiful inventions public. Four children, two boys and two girls, were the fruit of this marriage.

* It was especially the illness of his daughter, and the delicate health of his younger son, that led Watt to interest himself so deeply on this head. His work was entitled a *Description of a Pneumatic Apparatus, with Directions for procuring the Factitious Airs.— Translator.*

† Twenty years before the establishment of the Pneumatic Institution at Bristol, Watt already applied his chemical and mineralogical knowledge to improving the products of a pottery that he had established at Glasgow together with some friends, and of which he continued a partner to the end of his life.

Mrs. Watt died at the birth of a third boy, who did not survive. Her husband was then busy in the north of Scotland, with the plans for the Caledonian Canal. Why should I not be permitted to transcribe here with all their originality some lines from the journal to which he committed daily his most private thoughts, his fears, his hopes! Why should I not show him to you, after his misfortune, stopping on the sill of the door of his house where his *sweet welcomer* no longer awaited him; unable to re-enter those rooms, where he was no longer to find *the comfort of his life!* Perhaps so true a picture of deep grief would silence those systematic spirits who—without pausing at the thousands of striking contrary instances—deny that the qualities of the heart are possessed by any man whose intellect has been nurtured with the fruitful, the sublime, the imperishable truths of the exact sciences.

After remaining for some years a widower, Watt had again the happiness to find in Miss Mac Grigor a companion worthy of him by the variety of her talents, the soundness of her judgment, and the energy of her character.*

At the expiration of the patent granted him by Parliament, Watt, at the beginning of 1800, retired entirely from business.

His two sons succeeded him. Under the sensible direction of Mr. Boulton junior and the two young Messrs. Watt the manufactory at Soho prospered, and exhibited new and important developments. Even now it occupies the first rank in England among the establishments for constructing large machines. The second of the two sons, Gregory Watt, became known to the world in a brilliant manner, by his literary compositions, and by his geological labours. He died at the age of twenty-seven, in 1804, of a disease of the chest. This sad event overthrew the illustrious engineer. The tender attentions of his family and of his friends with difficulty succeeded in restoring some degree of calm to his broken heart. This very justifiable grief seems to explain the almost absolute silence which Watt maintained during the several latter years of his life. I am far from denying that it was without influence; but what occasion is there to seek for extraordinary causes, when already, under date of 1783, we read in a letter from Watt to his friend Dr. Black:—"Recollect well, that I have no desire to entertain the public with the experiments which I have made;"—when we also meet elsewhere, these very singular words in the mouth of a man who has filled the world with

* Mrs. Watt (Mac Grigor) expired 1832, at a very advanced age. She had endured the grief of surviving the two children that resulted from her marriage with Mr. Watt.

his renown:—"I know only two pleasures, idleness and sleep." This sleep, however, was very light; and let us add, that the least excitement sufficed to rouse him from his favourite idleness. All the objects that were presented to him gradually received from him a mental suggestion of change of form, of nature, or of construction, which would have rendered them capable of important applications. These conceptions, for want of opportunities of being produced, were lost to the world.* The following anecdote will explain my ideas.

A company at Glasgow had erected large buildings and powerful engines on the right bank of the Clyde, intended to carry water to all the houses in the town. When this work was completed, they perceived that there existed on the opposite shore a spring, or rather a natural filter, which gave the water evidently superior qualities. To change the site of the establishment, could not even be proposed; they therefore thought of leading a fixed conduit-pipe all across the river, along the bottom, the mouth of which should always be in the midst of potable water; but the construction of the wood-work to support such a pipe, on its muddy, changeable, and uneven bed, always covered with several metres of water, seemed to require too heavy an outlay. Watt was consulted. His solution was all ready: having some days before seen a lobster on the table, he sought and found how mechanism might, with the aid of some iron, form a series of articulations, which should have all the flexible mobility of the tail of crustacea; he therefore proposed an articulated conduit-pipe, susceptible of bending itself to all the present and future inflections of the bed of the river. According to the plans and designs of Watt, therefore, the Glasgow Company ordered this iron lobster-tail to be made, sixty centimetres (nearly two feet) in diameter, and above three hundred metres (1000 feet, English) in length; and its success was complete.

Those who had the happiness of being personally acquainted with him, do not hesitate to assert that, in his own house, the qualities of his heart shone even above those of the philosopher. An infantine candour, the greatest simplicity of manners, a love of justice carried beyond every scruple, an inexhaustible bene-

* There can be no doubt that Watt was deeply affected by his melancholy bereavement; but his mental energy was never impaired by it, nor his interest in science and literature weakened. Indeed there seems to be but little recollection of the lengthy silence above alluded to. The anecdote which follows respecting the lobster's tail, which he imitated on a large scale by a sort of ball-and-socket movement, shows that his inventive powers were still bright and fertile in 1811.—*Translator.*

volence, these are the virtues that have given rise to indelible recollections both in Scotland and England. Watt, although so moderate and so gentle, became irritated when in his presence an invention was not assigned to its true author; especially when any low flatterer wished to enrich him at other men's expense. In his eyes, scientific discoveries were the highest of all property. Whole hours of discussion did not seem too much to him, if the object was to do justice to diffident inventors, either robbed by plagiarists, or only forgotten by an ungrateful public.

Watt's memory might be cited as prodigious, even by the side of all that is related of this faculty in some highly endowed men. Its extent, however, was its least merit: it imbibed all that was of any value; and it entirely rejected, almost instinctively, the superfluities that it would have been useless to preserve.

The variety of knowledge possessed by our academician would be truly incredible, if not attested by many eminent men. Lord Jeffrey, in an eloquent biographical notice, happily characterised both the strong and subtle intelligence of his friend, when he compared it to the elephant's trunk, so wonderfully organised, that the animal uses it with equal facility either to "pick up a pin" or "to rend an oak."

Sir Walter Scott speaks of his countryman in the following terms, in the preface to *The Monastery* *:—

"It was only once my fortune to meet him, whether in body or in spirit it matters not. There were assembled about half a score of our Northern Lights. . . . . Amidst this company stood Mr. Watt, the man whose genius discovered the means of multiplying our national resources to a degree perhaps even beyond his own stupendous powers of calculation and combination; bringing the treasures of the abyss to the summit of the earth; giving the feeble arm of man the momentum of an Afrite; commanding manufactures to rise, as the rod of the prophet produced water in the desert; affording the means of dispensing with that time and tide which wait for no man; and of sailing without that wind which defied the commands and threats of Xerxes himself. This potent commander of the elements, the abridger of time and space, this magician whose cloudy machinery has produced a change on the world, the effects of which, extraordinary as they are, are perhaps only now beginning to be felt, was not only the profound man of science, the most successful combiner of powers and calculator of numbers as adapted to practical purposes, was not only one of the most generally well informed, but one of the best and kindest of human beings.

"There he stood, surrounded by the little band I have mentioned of northern literati, men not less tenacious, generally speaking, of their own fame and their own opinions than the National Regiments are supposed to be jealous of the high character which they have won upon service. Methinks I yet see and hear what I shall never see or hear again. In his eighty-fifth (*eighty-third?*) year,

* We have thought it better to give the whole passage from Sir Walter Scott, than to reproduce it from our author's French; nor have we adopted his omissions.— *Translator.*

the alert, kind, benevolent, old man had his attention at every one's question, his information at every one's command.

"His talents and fancy overflowed on every subject. One gentleman was a deep philologist — he talked with him on the origin of the alphabet as if he had been coeval with Cadmus; another, a celebrated critic—you would have said the old man had studied political economy and belles-lettres all his life; of science it is unnecessary to speak, it was his own distinguished walk. And yet, Captain Clutterbuck, when he spoke with your countryman, Jedediah Cleishbotham, you would have sworn he had been coeval with Claver'se and Burley, with the persecutors and persecuted, and could number every shot the dragoons had fired at the fugitive covenanters. In fact, we discovered that no novel of the least celebrity escaped his perusal, and that the gifted man of science was as much addicted to the productions of your native country (the land of Utopia aforesaid), in other words, as shameless and obstinate a peruser of novels as if he had been a very milliner's apprentice of eighteen."

If our associate had wished it, he could also have made himself a name among novelists. Among his intimate friends he seldom failed to improve on the terrible, moving, or burlesque anecdotes that he heard related. The minute details of his recitals, the proper names with which he strewed them; the technical descriptions he gave of the castles, the country houses, the forests, the caverns, to which the scene was successively transferred, gave to these impromptus such an air of veracity, that one could not entertain the slightest sentiment of distrust. One day, however, Watt was at a loss how to extricate his characters from the labyrinth into which he had imprudently thrown them. One of his friends perceived, by the uncommon number of pinches of snuff he took, that the narrator wished thereby to excuse frequent pauses, and gain time for reflection. He therefore addressed this indiscreet question to him: "Are you perhaps relating to us a story of your own creation?" "That doubt astonishes me," wittily replied the old man; "during the twenty years that I have had the happiness of passing my evenings with you, I have done nothing else! Is it possible that they really wished to represent me as emulous of Robertson or of Hume, whilst all my ambition was limited to follow, however far behind, the steps of Princess Scheherazade in the *Thousand and One Nights!*"

Each year, during a very short visit to London, or to other towns at a less distance from Birmingham, Watt examined minutely all the novelties that had appeared since his preceding visit. I do not except even the sight of the industrious fleas or the puppet-shows; for the illustrious engineer went to them with all the delight of a school-boy. While perusing, even at the present day, the itinerary of these annual excursions, we should find luminous traces of Watt's presence. At Manchester for example, we should see the hydraulic ram serving, according to his own proposition,

to raise the water of condensation from a steam-engine up to the reservoir feeding the caldron.

Watt generally resided on an estate near Soho called Heathfield, which he acquired about the year 1790. The filial veneration of my friend Mr. James Watt, for everything connected with his father's memory, procured for me, in 1834, the satisfaction of finding the library and the furniture at Heathfield in the same state in which the illustrious engineer left them. Another property on the picturesque banks of the river Wye, in Wales, offers to the tourist numerous proofs of the enlightened taste both of Watt and of his son, by the improvement of the roads, by the plantations, and by agricultural labours of all kinds.

Watt's health had become stronger with his years. His intellectual faculties continued in full vigour to his last moments. He thought at one time that they were declining, and adhering to the thought expressed on the seal that he had selected (an eye surrounded by the word *Observare*), he determined to clear up his doubts by self-observation; accordingly, when above seventy, we see him seeking the kind of study to which he should best have recourse for a trial, and distressed that no subject was new to him. He recollects at last that there is an Anglo-Saxon language, that it is a difficult language, and the Anglo-Saxon becomes the desired experimental means, — the facility which he finds in rendering himself master of it, proves to him how unfounded his apprehensions were.

Watt devoted his last days to the construction of a machine for copying promptly either statues or sculpture of any size with mathematical fidelity. This machine, of which we hope the arts will not be deprived, must have been well advanced. Many of its productions — already very satisfactory — may be seen in various private collections in Scotland and in England. The illustrious engineer had presented them in joke, as the first essays of a young artist entering the eighty-third year of his age.

It was not permitted to our associate to see the end of this eighty-third year. From the very beginning of the summer of 1819, some alarming symptoms defied all the powers of medicine. Watt himself was not deceived. He said to the numerous friends who visited him — "I am moved by the attachment that you show me, I hasten to thank you for it, as you see me arrived at my last illness." His son did not appear to him sufficiently resigned; whereupon he each day sought a new reason by which to point out to him with gentleness and tenderness, "all the motives of consolation that he might derive from the circumstances under which the inevitable event was about to occur." This sad event did in fact take place on the 25th of August, 1819.

Watt was buried by the side of the parish church of Heathfield, near Birmingham, in the county of Stafford. Mr. James Watt, whose distinguished talents, and whose noble sentiments delighted his father's heart for nearly twenty-five years, erected a splendid Gothic monument to him, and it now greatly adorns Handsworth Church.* In the centre there stands an admirable statue by Chantrey, the exact representation of the old man's noble features.

A second statue, also of marble, from the hands of the same sculptor, has been placed by filial piety in one of the halls of the brilliant university where, during his youth, the then unknown artist, though harassed by the corporation, received such flattering and well-deserved encouragement. Nor has Greenock forgotten that Watt was born there. The inhabitants have subscribed for a statue of the illustrious mechanic, to be placed in a fine library, built on a piece of ground generously given by Sir Michael Shaw Stewart; and there will be gathered the books that the town possessed, and the collection of scientific works that Watt had presented to the town during his life. This building has already cost 3500*l.* sterling (upwards of 87,000 frs. of our money), a considerable expense for which the liberality of Mr. Watt Junior has provided. A grand colossal statue in bronze, on a beautiful granite base, now adorns one of the angles of George's Square, at Glasgow; proving to all beholders, how much that capital of Scotch industry prides itself in having been the cradle of Watt's discoveries. Finally †, the gates of Westminster Abbey opened at the imposing voice of a host of subscribers; and a colossal statue of our co-academician, of Carrara marble, a master-piece of Chantrey's, the pedestal bearing an inscription by Lord Brougham, has become within these few years one of the principal ornaments of the English Pantheon. Doubtless a little coquetry was necessary to bring together the illustrious names of Watt, Chantrey, and Brougham on the same monument; but I can see nothing to blame in it: glory to the people who thus seize every opportunity of honouring their great men!

This inscription by Lord Brougham, put on the pedestal of the statue of our *confrère*, appears to us to be worthy of a place in these pages, devoted to the memory of one of the greatest geniuses that ever illustrated science and industry; we will reproduce it then literally, a translation shall follow:

* To a general reader this paragraph might convey an ambiguity; Watt died in his house at Heathfield, at the age of eighty-three years and seven months; and his remains are deposited in the chancel of the adjoining parochial church of Handsworth, near those of his excellent friend Miss Boulton. — *Translator.*

† Two years ago a statue of Watt was erected in Edinburgh. — *Translator.*

NOT TO PERPETUATE A NAME
WHICH MUST ENDURE WHILE THE PEACEFUL ARTS FLOURISH,
BUT TO SHOW
THAT MANKIND HAVE LEARNT TO HONOUR THOSE
WHO DESERVE THEIR GRATITUDE.
THE KING
HIS MINISTERS AND MANY OF THE NOBLES
AND COMMONERS OF THE REALM,
RAISED THIS MONUMENT TO
JAMES WATT,
WHO DIRECTING THE FORCE OF AN ORIGINAL GENIUS
EARLY EXERCISED IN PHILOSOPHIC RESEARCH,
TO THE IMPROVEMENT OF
THE STEAM-ENGINE,
ENLARGED THE RESOURCES OF HIS COUNTRY,
INCREASED THE POWER OF MAN,
AND ROSE TO AN EMINENT PLACE
AMONG THE MOST ILLUSTRIOUS FOLLOWERS OF SCIENCE
AND THE REAL BENEFACTORS OF THE WORLD.
BORN AT GREENOCK, MDCCXXXVI,
DIED AT HEATHFIELD, IN STAFFORDSHIRE, MDCCCXIX.*

There are, actually counted, five large statues erected in a short time to the honour of Watt. Must we acknowledge it?— this homage of filial piety, of public gratitude, has excited the ill-humour of some narrow minds, who, remaining stationary themselves, think they can arrest the march of centuries. If we believe them, some military men, some magistrates, some ministers (I must confess they have not dared to say all the ministers), would have a right to statues. I know not whether Homer, Aristotle, Descartes, Newton would appear to these new Aristarchi deserving of a bust; assuredly they would refuse the most unassuming medal to the Papins, the Vaucansons, the Watts, the Arkwrights, and other mechanics, unknown, perhaps, in a certain world, but whose renown will go on increasing from age to age with the progress of knowledge. When such heresies are brought forward in open daylight, we ought not to disdain combating them. It is not without reason that the public has been called a sponge of prejudices; now prejudices are like noxious weeds, the slightest effort suffices to extirpate them on their first appearance; but, on the other hand, they resist if they are allowed time to grow, to expand, to seize by their numerous organs all that is suited to their nature.

If this discussion should wound the self-love of some people, I must remark that it has been provoked. Have the learned men of our own times uttered complaints at not seeing any of the great authors, whose inheritance they cultivate, figure in those long

* The French translation, for obvious reasons, is omitted.— *Translator.*

ranges of colossal statues, which authority pompously raises on our bridges and in our public squares? Do they not know that these monuments are fragile, that storms upset and destroy them, that frost suffices to spoil their outlines, and to reduce them to amorphous blocks?

Their sculpture and their painting is the press. Thanks to that admirable invention, when the works which science or imagination produces possess real merit, they may defy time and political revolutions. Neither the exigences of the Exchequer, nor the inquietudes and terrors of despots, could prevent those productions from penetrating beyond the best-guarded frontiers. A thousand ships will carry them, in various shapes, from one hemisphere to the other. They will be read in Iceland and in Van Diemen's Land at the same time. They will be read at evening meetings in the humble cottage, they will be read in brilliant assemblies in palaces. The author, the artist, the engineer are known, appreciated, by the whole world, by that which there is in man of most noble, of most elevated: by the soul, by the thoughts, by the intellect. How foolish must that man be who, placed on such a theatre, should be detected in wishing that his lineaments were preserved by the chisel of a David *, to be some day exposed to the glances of idlers taking their walk. Such honours, I repeat it, need not be envied by the learned man, by the author, or by the artist; but they ought not, on any account, to allow themselves to be declared unworthy of them. Such, at least, have been the thoughts that lead me to submit the following discussion to your judgment.

Is it not a truly strange circumstance, that these vain pretensions that I am combating should have been raised merely on account of these five statues, not one of which cost a single obolus to the public treasury? Far from me, however, to take advantage of this inconsiderateness. I prefer taking the question in a more general point of view, such as it was laid down: the pretended pre-eminence of arms over letters, over science, over art; for we must not deceive ourselves — if magistrates and administrators have been mentioned together with military men, it was only as a passport.

* It is uncertain whether the noted Jacques Louis David, or Pierre Jean David is here meant; for though the former is generally known as a painter only, he proposed to construct a huge colossus in honour of the people, out of the ruins of royal statues; and of this he made a model. But we could have wished that our author's taste had prevented his intruding the truisms in this and in the tirade which follows; at least, the biography of the enriched and greatly honoured Watt hardly appears to be a fit peg whereon to hang so laboured a declamation. Even now, one of the finest line-of-battle ships in the British fleet is the JAMES WATT; still, we admit, the best records of an eminent man are certainly his works. — *Translator.*

The shortness of the time allowed me for this discussion, imposes on me the duty of being methodical. In order that my sentiments may not be mistaken, I will at once declare aloud that independence, that national liberty, are in my opinion the greatest possible good; that to defend them against foreigners, or against internal enemies, is our first duty; and that to have defended them at the cost of our blood, is the highest title to public gratitude. Raise, raise splendid monuments to the memory of the soldiers who fell on the glorious ramparts of Mayence, on the immortal fields of Zurich, of Marengo, and certainly my offering shall not be waited for; but do not require me to do violence to my reason, to the sentiments that Nature has implanted in the human heart; do not hope that I will ever consent to place all military services on the same level.

What Frenchman possessed of a heart, even in the reign of Louis XIV., would have sought for an example of courage either among the scenes of cruelty in the Dragonnades, or among the whirlwinds of flame that devoured the towns, the villages, and the rich country of the Palatinate?

Not long since, after a thousand prodigies of patience, of cleverness, of bravery, our valiant soldiers penetrated into the half-destroyed Saragossa, and reached the door of a church where the preacher was still making the ears of the resigned crowd ring with these magnificent words: "Spaniards, I am going to celebrate your funerals!" I know not, but I think that at such a moment the true friends of our national glory, comparing the various merits of the conquerors and the vanquished, would willingly perhaps have inverted the address!

But I consent to your laying aside the question of morality. Submit the personal claims of some gainers of battles to the crucible of a conscientious analysis; believe me, that even if you make an equitable partition by chance (a sort of ally for whom one always makes allowance, as being dumb), many pretended heroes will appear to you very unworthy of that pompous title.

If it were found requisite, I would not recoil from a detailed examination; I, who in a purely academic career, can have had but little opportunity of collecting correct documents on such a subject,—I could, for example, cite in our own annals a recent battle, a battle gained, the official report of which describes it as having been foreseen and calmly prepared, with the most consummate ability; but which, in reality, was the result of a sudden rush on the part of the soldiers, without any order from the Commander-in-Chief to whom the honour was assigned, without his having been there, without his having known of it!

To escape from the commonplace reproach of incompetence, I

will call on some military men themselves to aid in supporting the philosophic thesis which I maintain. It will be seen what enthusiastic and enlightened appreciators they were of intellectual labours; it will be seen that in their inner mind, these never held a second rank. Obliged to restrict myself, I will try to make high renown supply the deficiency of number and novelty: I will cite Alexander, Pompey, Cæsar, and Napoleon!

The Macedonian conqueror's admiration of Homer is historical. Aristotle at his desire undertook the task of revising the text of the Iliad. That corrected copy became a cherished book; and when, in the centre of Asia, amidst the spoils of Darius, a magnificent casket was found, enriched with gold, pearls, and precious stones, which seemed to excite the covetousness of his highest officers, the conqueror of Arbela exclaimed: "Let that be reserved for me; it shall contain my Homer. It is the best and most faithful counsellor I have in my military affairs. Besides, it is but just that the richest production of art should preserve the most precious work of the human mind."

The sacking of Thebes had already shown, still more clearly, the unlimited respect and admiration that Alexander entertained for letters. Only one family out of that populous city escaped death and slavery: this was the family of Pindar. Only one house remained intact amidst the ruined temples, palaces, and private dwellings: this was the house where Pindar was born, not Epaminondas!

When Pompey, after finishing the war against Mithridates, went to visit the celebrated philosopher Posidonius, he prohibited the lictors from knocking at the door with their sticks, as was the custom. Thus, says Pliny, were the fasces of the man who had seen the East and the West prostrated before him, lowered before the humble dwelling of a learned man!

Cæsar, who may also be claimed as a man of letters, allows us to perceive, in at least twenty places in his immortal Commentaries, what rank was occupied in his own esteem by the various faculties with which nature had so liberally endowed him. How brief he is, how rapid in relating combats and battles! See, on the contrary, whether he thinks any detail superfluous in the description of the temporary bridge by means of which his army crossed the Rhine. It is because success depended here on the conception, and the conception was exclusively his own.

It has also been already remarked, that the part which Cæsar by preference attributed to himself in the events of the war, that of which he seems to have been most proud, was a moral influence. *Cæsar harangued his army,* is constantly the first phrase with which he begins, when describing a battle gained. And *Cæsar did not*

arrive soon enough to talk to his soldiers, to exhort them to conduct themselves well, is the general accompaniment of the recital of a surprise or of a momentary repulse. The *general* frequently undertakes to efface himself in presence of the *orator.* And the judicious Montaigne remarks:—"His language, truly, in many places does him notable service!"

Meantime, without transition, without even insisting on the well known exclamation of Frederic the Great: "*I would rather have written the Century of Louis XIV. by Voltaire, than have gained a hundred battles,*" I come to Napoleon. As we must hasten on, I will not recall the celebrated proclamations, written under the shade of the Egyptian Pyramids by the *Member of the Institute,* Commander-in-Chief of the army of the East; nor the treaties of peace, in which monuments of art or of science were the price of the vanquished people's ransom; nor the profound esteem which the general, become emperor, never ceased to feel for the Lagranges, the Laplaces, the Monges, the Berthollets, nor the riches nor the honours which he showered down upon them. An anecdote, little known, will lead more directly to my aim.

Everybody remembers the decennial prizes. The four classes of the Institute had sketched out rapid analyses of the progress made in the sciences, letters, and arts. The presidents and the secretaries were to be called in succession to read them to Napoleon, in the presence of the great dignitaries of the Empire, and the Council of State.

On the 27th of February, 1808, it came to the turn of the French Academy. As may be easily supposed, the assembly on that day was even more numerous than usual: who does not think himself a judge in matters of taste? Chenier reads. He is listened to with attentive silence: but all at once he is interrupted by the emperor, who, putting his hand on his heart, his body leaning forward, his voice affected by a visible emotion, exclaimed: "It is too much, too much, Gentlemen, you overpower me; words fail me in which to express my gratitude!"

I leave you to imagine the deep surprise of the many courtiers who witnessed this scene; those men who from flattery to flattery had come at last to say to their master, and without his appearing astonished at it:—"When God had created Napoleon, he felt the want of repose!"

But what then were the words that went so exactly, so directly to the heart of Napoleon? These words were the following:—

"In camps where, far from the calamities of the interior, national glory was unalterably preserved, another style of eloquence arose, unknown until then to modern nations. We must

even acknowledge, that when we read in ancient authors harangues from the most renowned leaders, we are often tempted to admire only the talent of the historians in them. But here, it is impossible to doubt; the monuments exist: history has only to collect them together. It was from the armies of Italy that those beautiful proclamations emanated, in which the conqueror of Lodi and of Arcoli, created at the same time a new system of warfare, and a new style of military eloquence, of which he will for ever remain the model."

On the 28th of February, the day after the celebrated sitting that I have just described, the *Moniteur*, with its *known fidelity*, published an answer from the emperor to Chenier's discourse. It was cold, laconic, unmeaning; it had, in short, all the characteristics that other people would say are those of an official document. As to the incident that I recorded, there was no allusion to it; a wretched concession to predominant opinions, to the susceptibilities of a military etiquette! The master of the world, to use Pliny's expression, ceding for a moment to his inward feeling, had not the less bowed his fasces to the literary title awarded to him by an Academy.

These reflections on the comparative merits of the man of letters and the man of arms, although not chiefly suggested by what is said, by what is done under our ocular experience, would not be inapplicable to the country of James Watt. I travelled not long since through England and Scotland. The good will with which I was received, authorised questions on my part, as dry, as pointed, as direct, as might, under other circumstances, have come only from the president of a commission of inquiry. Already fully preoccupied with the obligation I should be under, at my return, to give a judgment on the illustrious mechanic; already feeling uneasy at the solemn character of the meeting before which I am speaking, I had prepared the following question: "What do you think of the influence that Watt had on the riches, on the power, and on the prosperity of England?" I do not exaggerate in saying that I addressed this question to upwards of a hundred persons belonging to all classes of society, to all varieties of political opinions, from the most violent radicals to the most obstinate conservatives. The answer was always the same: every one placed the services of our academician above all comparison; each man quoted, besides, the discourses pronounced at the meeting in which the Westminster statue was voted, as the faithful and unanimous expression of the sentiments of the English nation. What did these discourses say?

Lord Liverpool, Prime Minister of the Crown, calls Watt, "one of the most extraordinary men that England ever produced,

one of the greatest benefactors to the human race." He declared that "his inventions have augmented the resources of his country and of the whole world, to an incalculable degree." Then, considering the question in a political point of view, he added,—"I have lived at a time, when the success of a war depended on the possibility of pushing our fleets out of port without loss of time; contrary winds prevailed during whole months, and would have entirely upset the intentions of government. Thanks to the steam-engine, such difficulties have disappeared for ever." *

"Direct your attention," Sir Humphry Davy exclaimed, "to the metropolis of this powerful empire, to our towns, to our villages, to our arsenals, to our manufactories; examine our subterranean cavities, and the works accomplished on the surface of the globe; contemplate our rivers, our canals, the seas which bathe our shores; you will everywhere find proofs of the eternal benefits conferred by that great man."

The illustrious President of the Royal Society also said: "The genius exerted by Watt in his admirable inventions, has contributed more to show the practical utility of science, to enlarge the power of man over the material world, to multiply and to spread the conveniences of life, than the efforts of any other man of modern times." Finally, Davy does not hesitate to place Watt above Archimedes!

Then Huskisson, Minister of the Board of Trade, divesting himself for a moment of the character (*qualité*) of an Englishman (?), proclaims that compared in their bearings on the happiness of the whole human species, Watt's inventions would still appear to him to deserve *the highest admiration.* He explains in what manner the economy of labour, the indefinite multiplication and cheapness of industrial products, contribute to excite and to spread knowledge. He said; "The steam-engine is not only, in the hands of man, the most powerful instrument they use to alter the face of the physical world; it acts also as a moral and irresistible lever for pushing on the great cause of civilisation."

From this point of view, Watt appeared to him in a distinguished rank among the benefactors of humanity. As an Englishman he does not hesitate to say that without Watt's creations the British nation could not have stood the immense expenses of its recent war with France.

The same idea may be observed in the discourse of another member of Parliament, in that of Sir James Mackintosh: see whether he expresses himself in less positive terms. "It was

* It should be observed that during the wars of which Lord Liverpool had any cognisance, steam had not been applied to this purpose.—*Translator.*

the inventions of Watt that enabled England to sustain the severest, the most dangerous conflicts that she was ever engaged in." Everything considered, Mackintosh declares, without hesitation, that "no man has had more evident claim than Watt to the homage of his country, to the respect and veneration of future ages."

I will now give some numerical estimates, some numbers, which to my mind are more eloquent still than the several passages which I have been quoting.

Mr. Boulton, junior, announced that in the year 1819, the manufactory of Soho alone had already made Watt's engines equal to the labour of a hundred thousand horses; that the saving arising from the substitution of these machines for animal power amounted to seventy-five millions of francs (three millions sterling) per annum. In England and Scotland, at that time, there existed upwards of ten thousand steam engines. They did the work of five hundred thousand horses, or of between three and four millions of men; with an annual saving of three or four hundred millions of francs (twelve or sixteen millions sterling). And these results must by this time be more than doubled.

I have thus abridged what was thought of Watt by the ministers, the statesmen, the learned men, and the industrial men—the best able to appreciate him. Gentlemen, the creator of six or eight millions of workmen, indefatigable and assiduous workmen, among whom authority will never have to repress combination or riot, workmen on five centimes per day (one halfpenny); this man, who by brilliant inventions, gave England the means of maintaining a desperate struggle, during which its very nationality was at stake, this new Archimedes, this benefactor of the whole human race, whose memory will be blessed by future generations— what was done to honour him during his life?

The peerage is in England the first of its dignities, its highest reward. You will naturally suppose that Watt was made a peer.

Such a thing was not even thought of!

To speak honestly, so much the worse for the peerage, which would have been honoured by the name of Watt!

Such a neglect, however, in a nation so justly proud of its great men, might well astonish me. And when I inquired the cause, what do you think they answered? "The dignities of which you speak are reserved for officers of the army and navy, for influential orators in the House of Commons, for members of the nobility. *It is not the fashion* (I do not invent, I quote precisely), it is not the custom, to grant them to learned men, to literary men, to artists, to engineers!" I know well that it was not the the fashion under Queen Anne, since Newton was not made a peer of

England.* But after a century and a half of progress in science and in philosophy, when each of us during the short course of his life has seen so many wandering kings cast off, proscribed, succeeded on their thrones by soldiers without genealogy, sons of their sword, was it not allowable to think that it had become obsolete to divide men into folds; that none would any longer say to their faces, as in the inflexible code of the Pharaohs—"Whatever be your services, your virtues, your knowledge, not one of you shall overstep the boundary of your caste;" that a foolish custom, in short, since such a custom exists, should no longer blot the institutions of a great nation!

Let us trust to the future. A time will come when the science of destruction will bow before the arts of peace; when the genius which multiplies our powers, which creates new products, which spreads comfort among masses of people, will occupy that place in the general esteem of mankind, that reason and good sense assign to it already.

Watt will then appear before the grand jury of the two worlds. Every one will see him, aided by his steam-engine, penetrate in a few weeks into the bowels of the earth to depths where, before him, we could not have arrived without a century's most painful efforts; he will dig spacious galleries there, and will clear them in a few minutes of the immense volumes of water that used to inundate them daily; he will drag from a virgin soil the inexhaustible riches that nature deposited there.

Uniting delicacy with power, Watt will twist with equal success the enormous strands of the colossal cable by which the man-of-war moors itself in the midst of the chafed ocean—and the microscopic filaments of that lace, of that aërial web, which forms so favourite a portion of the various dresses introduced by fashion.

A few oscillations of this same machine will restore to agriculture vast swamps; thus fertile countries will be rescued from the periodical and fatal miasma that used to be fostered there by the burning summer-suns.

The great mechanical powers that we used to have to seek in mountainous regions, at the foot of large water-falls, now, thanks to Watt's discovery, will arise at will, in a compact form, and without annoyance in the midst of towns, in every floor of a house.

* The whole truth should have been told. Newton, though unfortunately not made a peer, was never hidden under a bushel. He was knighted by Queen Anne, and *courted* by King George I. and by the Princess of Wales, afterwards Queen Caroline. He was President of the Royal Society, a Member of Parliament, and Master of the Mint; and at his interment the pall was supported by the Lord High Chancellor, the Dukes of Montrose and Roxburgh, and the Earls of Pembroke, Sussex, and Macclesfield. Moreover our author seems to have excluded the host of lawyer-peers from the class "learned men."—*Translator.*

The intensity of this power will vary according to the will of the mechanic; it will not depend as formerly on the most inconstant of natural causes: on atmospheric meteorology.

The various branches of a manufactory can be united in one common area, and under one roof.

The industrial products, whilst undergoing improvement, will also be reduced in price.

The population well-fed, well-dressed, well-warmed, will increase rapidly; it will cover every part of the territory with elegant habitations; even those parts that might justly be called the steppes of Europe, and which from the aridity of ages seemed to be condemned to remain the exclusive domain of wild beasts.

In a few years hamlets will become important cities; in a few years boroughs, such as Birmingham, where there used to be scarcely thirty streets, will rise to be ranked among the largest cities, the handsomest and the richest of a powerful kingdom.

Installed on board ship, the steam-engine will replace a hundredfold the efforts of the triple, of the quadruple banks of rowers, from whom our ancestors required a degree of labour classed among the punishments of the worst criminals.

By the aid of a few tons of coals, man will conquer the elements; he will laugh at calms, at contrary winds, at storms.

Passages from one country to another will become more rapid: the time of the steam-boat's arrival can be foretold as correctly as that of a public land conveyance; you will no more go to the seashore for weeks, or even whole months, your heart a prey to cruel anguish, seeking with an anxious eye along the horizon, for the uncertain glimpse of the ship that is to restore to you a father, a mother, a brother, or a friend.

To conclude, the steam-engine, dragging in its train some thousands of travellers, will run along the railways much faster than the best blood horse bearing only his light jockey along the race-course.

There, Gentlemen, is a very brief sketch of the benefits bequeathed to the world by the machine, the germs of which Papin had deposited in his works, and which, after so many ingenious efforts, Watt has brought to an admirable perfection.* Posterity

* A translator should not, perhaps, enter the lists, but he may intrude a remark. It is difficult to opine why our author should bestir himself so eagerly to give Watt the composition of water, and yet impair his grand claim to universal homage by foisting in the names of Rivault, De Caus, and others as inventors: the early engines were mere toys and pumps, and therefore foreign to the marvellous and almost animated machine which is now in use. Some of Watt's excogitations and contrivances, the product of lengthy intellectual

certainly will not weigh them with other labours that have been too much vaunted; and whose real influence at the tribunal of reason, will always remain circumscribed to a circle of a few individuals, and a short compass of years.

They used formerly to appeal to the age of Augustus, then to the age of Louis XIV. Some eminent minds have already maintained that it would be justice to speak of the age of Voltaire, of Rousseau, of Montesquieu. As to myself, I do not hesitate to announce, that when to the immense services already rendered by the steam-engine, all the wonders are added that it still promises, grateful nations will also speak of the ages of Papin and of Watt!

ACADEMICAL TITLES WITH WHICH WATT WAS INVESTED.

A biography of Watt, intended to make part of our collection of memoirs, would certainly be incomplete if it did not contain a list of the academical titles with which the illustrious engineer was invested. This list, moreover, will occupy only a few lines: —

struggles, are slurred over, while others are not even alluded to; and the difficulties he had to combat with in metallurgy are altogether omitted.

We ought to be cautious in attaching an undue value to mere sagacious surmises, unsupported by legitimate proof; for notions may arise without being brought to bear; and simultaneous ideas may be formed without the parties being indebted to each other. M. Arago cannot tell whether De Caus actually made an engine; but surely he ought, as a self-constituted historical umpire, to have consulted the published *Travels of Cosmo III.* (*Grand Duke of Tuscany*) *in England in the reign of Charles II.*, and he would there have found that the Marquis of Worcester actually did make one (*see note to page* 536.). Now for all that is admirable in the structure of the mighty piece of mechanism, and really marvellous in its application, Watt was not a mere improver, but a highly-gifted inventor. We therefore insist that, to all its useful intents and purposes, the present STEAM-ENGINE is a British production. Thus in transcendental science, although preceding and contemporary philosophers had made conjectures on the subject that did not differ widely from truth, Newton, by an inductive ascent through a train of abstruse investigations to its principle, and thereby detecting and expounding its laws, is justly recognised as the author of the sublime hypothesis of GRAVITATION. Who will deny to Herschel the merit of discovering the planet Uranus, since Flamsteed had previously observed it as a star? Or still later, because some philosophers thought that there might exist a planet exterior to Uranus, who would deny the palm to those whose energies were awakened by the orbital tremblings of that outer body to the splendid discovery of Neptune? In reality, De Caus, Worcester, and Papin may be placed with respect to Watt, as Gilbert, Kepler, and Hooke are to Newton; or as Lambert, De Zach, and Bode will be to Le Verrier and Adams.— *Translator.*

Watt became:

Fellow of the Royal Society of Edinburgh	- in 1784;
Fellow of the Royal Society of London	- in 1785;
Fellow of the Society of Batavia - -	- in 1787;
By a spontaneous and unanimous vote, the Senate of the University of Glasgow awarded to Watt the honorary degree of Doctor of Laws - - - -	- in 1806;
Correspondent of the Institute - -	- in 1808;
The Academy of Sciences of the Institute paid Watt the highest honour there is in its awards, by naming him one of its eight Foreign Associates - -	- in 1814.

APPENDIX.

RETRANSLATION OF AN HISTORICAL NOTE BY LORD BROUGHAM, ON THE DISCOVERY OF THE COMPOSITION OF WATER.

There is no doubt that in England, at least, the researches respecting the composition of water originated in Warltire's experiments related in the fifth volume of Priestley.* Cavendish cites them expressly as having given him the idea of his work (*Phil. Trans.* 1784, p. 126.) Warltire's experiments consisted in the combustion of a mixture of oxygen and hydrogen, by means of the electric spark, and in closed vessels. Two results were reported therefrom: 1. a perceptible loss of weight; 2. the precipitation of some humidity on the sides of the vessels.

Watt inadvertently said in the note to page 332. of his Memoir (*Phil. Trans.* 1784), that the aqueous precipitation was observed for the first time by Cavendish; but Cavendish himself declares, p. 127., that Warltire had perceived the slight aqueous deposit, and quotes on this subject, Priestley's fifth volume. Cavendish could not ascertain any loss of weight. He remarks that Priestley's essays *had led him to the same result* †, and adds that the humidity which was deposited con-

* Warltire's letter, dated Birmingham, 18th April, 1781, was published by Dr. Priestley in the second volume of his *Experiments and Observations relating to various branches of Natural Philosophy, with a continuation of the Observations on Air,* forming, in short, the fifth volume of the *Experiments and Observations on different Kinds of Air,* published at Birmingham in 1781. — (*Note by Mr. Watt, jun.*)

† The note by Mr. Cavendish to p. 127., seems to imply that Priestley had not perceived any loss of weight; but I do not find this assertion anywhere in the Memoirs of the Birmingham chemist.

Warltire's earliest experiments on the combustion of gas were made in a copper globe which weighed 398 grammes, and the volume was 170 centilitres. The author wished "to decide whether heat is, or is not heavy."

Warltire first describes the method of mixing the gases, and of adjusting the scales; he then says, "I always carefully weighed the vessel filled with common air, so that the difference of weight after the addition of the inflammable air enabled me to judge whether the mixture had been effected in the desired proportions. The passage of the electric spark rendered the globe hot. After it had cooled by exposure to the air of the room, I suspended it again on the scales. I always found a loss of weight, but there were differences between one experiment and another. The mean loss was 129 milligrammes."

Warltire continues as follows: "I have exploded my airs in glass vessels since

tained no impurity (literally no particle of soot or of *sooty matter*). After a great number of trials, Cavendish perceived that if a mixture of common and of inflammable air is ignited, a mixture formed of 1000 measures of the former and 423 of the latter, "about one fifth of the common and nearly the whole of the inflammable air lose their elasticity, and *form by their condensation* the dew that covers the glass. . . . . On examining the dew, Cavendish found that it consisted of pure water. . . . . He thence concluded that nearly all the inflammable air and about one sixth of the common air *are turned into pure water*."

In a similar way, Cavendish burned a mixture of inflammable air, and dephlogisticated air (or hydrogen and oxygen); the fluid that was precipitated was always more or less acid, according as the gas burned with the inflammable air contained more or less phlogiston. The acid thus engendered was nitric acid.

Mr. Cavendish ascertained that nearly the whole of the inflammable air and the dephlogisticated air are *converted into pure water;* also, that if those airs could be obtained in a perfectly pure state, the whole would be *condensed*." If common air and inflammable air do not yield any acid when they are burnt, it is, according to our author, because the heat is not then intense.

Cavendish declares that his experiments, except in as far as they relate to the acid, were made in the summer of 1781, and that Priestley was aware of them. He adds, "One of my friends *gave some account* of them to Lavoisier, in the course of last spring (the spring of 1783), and also of the result that I had inferred from them, that is to say, that dephlogisticated air is water deprived of its phlogiston. But at that time, Lavoisier was so far from thinking that such an opinion was legitimate, that until the moment when he determined to repeat the experiments himself, he felt some difficulty in believing that nearly the whole of the two airs could be converted into water."

The friend alluded to in the preceding passage was Dr., since become Sir Charles Blagden. It is a remarkable circumstance, that this passage

I saw you recently do so yourself (Priestley), and I have observed, AS YOU DID, that however dry and clean the vessel might be before the explosion, it was afterwards covered with dew and a black *sooty substance*."

In comparing all these claims, does not the merit of having first perceived the dew belong to Priestley?

In the few remarks that Priestley has added to his correspondent's letter, he confirms the loss of weight, and adds, "Still I do not think that the bold opinion of the latent heat of bodies entering as a sensible part of their weight, can be admitted without making experiments on a larger scale. If that is confirmed it will be a very remarkable fact, and one that will do infinite credit to Warltire's sagacity."

And Priestley continues, "We must say also, that at the moment when he (Warltire) saw the dew on the interior surface of the closed glass vessel, he said that it confirmed an opinion which he had long entertained, the opinion that common air parts with its humidity when it is phlogisticated."

It is evident then that Warltire explained the dew by the simple mechanical precipitation of the hygrometric water contained in common air. — (*Note by Mr. Watt, jun.*)

in the work of Mr. Cavendish should not have formed part of the original Memoir that was presented to the Royal Society. The memoir seems to be written in the author's own hand-writing; but the paragraphs 134 and 135 were not there at first; they are added, and an indication is given as to where they belong; the writing is no longer that of Cavendish; these additions are in the hand-writing of Blagden. And it must have been he who gave all the relative details to Lavoisier, for it is not said that Cavendish held any direct correspondence with him.

The date of the reading of Cavendish's Memoir was the 15th of January, 1784. The volume of the *Phil. Trans.* of which this memoir form a part, did not appear till about six months after.

Lavoisier's Memoir (*volume of the Academy of Sciences for* 1781)* had been read in November and December, 1783. Various additions were made to it afterwards. The publication took place in 1784.

This Memoir contained a description of the experiments of June 1783, at which Lavoisier announces that Blagden was present. Lavoisier adds that this English physicist informed him "that Cavendish having already burnt inflammable air in closed vessels, had obtained a very sensible quantity of water;" but he nowhere says that Blagden informed him of the conclusions that Cavendish had inferred from those same experiments.

Lavoisier declares, in the most express manner, that the weight of the water is equal to that of the two gases that were ignited, unless, contrary to his own opinion, a sensible weight be assigned to the heat and to the light that were disengaged during the experiment.

This account does not agree with that of Blagden, which, according to all probability, was written as a refutation to Lavoisier's relation, after the reading of Cavendish's Memoir, and before the volume of the Academy of Sciences had reached England. This volume appeared in 1784, and assuredly it could not have reached London, either when Cavendish read his paper to the Royal Society, or still less when he wrote it. We must remark, besides, that in the passage of Cavendish's manuscript Memoir, in the hand-writing of Blagden, there is only one communication of experiments alluded to: one communication to Priestley. The experiments are there said to have been made in 1781; but there is no mention of the date of the communication. Nor are we informed whether the conclusions inferred from those experiments, and which, according to Blagden, were communicated by him to Lavoisier in the summer of 1783, were equally included in the communication made to Priestley. This chemist, in his Memoir written before the month of April, 1783, read in June of the same year, and quoted by Cavendish, says nothing of the theory of the latter, although he quotes his experiments.

Several propositions flow from the preceding facts: —

1. Cavendish, in the Memoir that was read to the Royal Society the 15th January 1784, describes the capital experiment of the combustion

* The date 1781 appears to be a clerical error for 1784. — *Translator.*

of the oxygen and hydrogen in closed vessels, and cites water as the product of this combustion;

2. In the same Memoir Cavendish deduces from these same experiments that the two forementioned gases are transformed into water;

3. In an addition by Blagden, made with the consent of Cavendish, the date of the summer of 1781 is assigned to the experiments of the latter; a communication to Priestley is quoted, without specifying the epoch, without mentioning conclusions, without stating even when those conclusions occurred to the mind of Cavendish. This must be regarded as a *most material* omission;

4. In one of the additions made to the Memoir by Blagden, Cavendish's conclusion is related in the following words: Oxygen gas is water deprived of its phlogiston. This addition is posterior to the arrival of Lavoisier's Memoir in England.

It may moreover be remarked, that in another addition to Cavendish's Memoir, written by the hand of that chemist, and which is certainly posterior to the arrival of Lavoisier's Memoir in England, Cavendish distinctly asserts for the first time, as in Lavoisier's hypothesis, that water is a compound of oxygen and hydrogen. Perhaps no essential difference will be found between this conclusion and the one which Cavendish had asserted before, that oxygen gas is water deprived of its phlogiston, for to render them identical it will suffice to look upon phlogiston as hydrogen; but to say of water that it consists of oxygen and hydrogen, is, certainly embracing a neater and less equivocal conclusion. I add, that in the original part of his Memoir, in that which was read to the Royal Society before the arrival of Lavoisier's Memoir in England, Cavendish thinks it more correct to consider inflammable air as phlogisticated water than as pure phlogiston."—(P. 140. *Phil. Trans.* for 1784.)

Now let us see what part Watt acted. The dates will here be of importance.

It appears that Watt wrote a letter to Dr. Priestley, on the 26th of April, 1783, in which he discusses the experiment of igniting the two gases in closed vessels, and that by it he came to the conclusion that water is composed of dephlogisticated air and phlogiston, both of them deprived of a part of their latent heat.*

Priestley deposited the letter in the hands of Sir Joseph Banks, with a request that it should be read at one of the earliest meetings of the

* We may feel quite safe in deducing from the inedited correspondence of Watt, that he had already formed his theory on the composition of water in December, 1782, and probably earlier. At all events, in his Memoir of the 21st of April, 1783, Priestley declares that, before his own experiments, Watt had entertained the idea that steam might be transformed into permanent gases.—(P. 416. *Phil. Trans.*, 1783.)

Watt himself, in his Memoir (p. 335. *Phil. Trans.*, 1784), declares that during several years he had entertained an opinion that air is a modification of water, and he makes known in detail the experiments and the reasonings on which this opinion rested.—(*Note by Mr. Watt, jun.*)

Royal Society. But Watt afterwards wished the reading to be deferred, in order to have time to see how his theory agreed with some of Priestley's recent experiments. In short, the letter was not read till April, 1784.* This letter was incorporated by Watt in a Memoir addressed to Deluc, dated 26th Nov. 1783.† Many new observations, and new reasonings, were introduced into this Memoir, although nearly the whole of the original letter was preserved in it, and in the printing it was distinguished from the additions by inverted commas. The important deduction, previously quoted, will be found in the part thus marked. We read also that the letter was communicated to several Fellows of the Royal Society when it reached Priestley in April, 1783.

In Cavendish's Memoir, such as it was when read, there was no allusion to Watt's theory; but it is mentioned in an addition, entirely in the handwriting of Cavendish, posterior to the reading of Watt's letters (*Phil. Trans.* 1784, p. 140.). In this addition Cavendish gives his reasons for not liking to render his conclusions complicated as Watt did, with considerations relative to the disengagement of latent heat; but it leaves us in doubt on the question as to whether the author was ever aware of the letter to Priestley of April, 1783, or whether he only saw the letter dated 26th of November, 1783, and read the 29th of April, 1784; on which it is requisite to remark that the two letters appeared in the *Philosophical Transactions* united in one. The letter to Priestley of the 26th April, 1783, remained some time (two months according to Watt's Memoir,) in the hands of Sir Joseph Banks and other Fellows of the Royal Society, during the spring of 1783. This is what we learn from the circumstances related in the note at p. 330. It seemed difficult to suppose that Blagden, Secretary of the Society, did not see the Memoir. Sir Joseph Banks must have given it to him, since he intended it to be read at the meeting (*Phil. Trans.* 1784, p. 330, note). Let us add that as the letter was preserved in the archives of the Society, it was in charge of Blagden, the Secretary. Would it be possible to suppose that the person whose hand wrote the remarkable passage already quoted, relative to a communication made to Lavoisier in June, 1783, of the conclusions that Cavendish had come to, would not have told Cavendish that Watt had come to those conclusions at the latest in April 1783? The conclusions are identical, with the mere difference that Cavendish calls dephlogisticated air water deprived of its phlogiston, and that Watt affirms water to be a union of dephlogisticated air and phlogiston.

We must remark that in Watt's theory there is the same uncertainty, the same vagueness, which we have already observed in that of Cavendish, and that it also proceeds from the use of the term phlogiston, which was

* The letter to Priestley was read the 22nd of April, 1784.

† Undoubtedly the Genevese physicist, then in London, received it at the time. It remained in his hands until Watt heard of the reading of Cavendish's Memoir at the Royal Society. From that moment my father made all possible arrangements for the Memoir addressed to Deluc and the letter of the 26th of April, 1783, addressed to Dr. Priestley, being read immediately at the Royal Society. This reading, claimed by Watt for the Memoir addressed to Deluc, was dated 29th of April, 1784.— (*Note by Mr. Watt, jun.*)

not well defined.* In Cavendish we cannot decide whether phlogiston is merely inflammable air, or whether this chemist is not rather inclined to consider a combination of water and phlogiston as inflammable air. Watt says expressly, even in his Memoir of the 26th November, 1783, and in a passage that does not form part of the April letter in 1783, that inflammable air, according to his ideas, contains a small quantity of water and a great deal of elementary heat.

These expressions from two such eminent men, should be regarded as indicating a certain degree of hesitation, relative to the composition of water. If Watt and Cavendish had had a precise idea that water results from the union of two gases deprived of their latent heat, from the union of the bases of inflammable air and of dephlogisticated air; if this idea had been accompanied in their minds by as much clearness as in that of Lavoisier, they would certainly have avoided the uncertainty and obscurity which I have pointed out.†

As far as relates to Watt, the following are the new facts that we have succeeded in establishing.

1. There is no proof that anybody had given, in a written document, anterior to Watt, the present theory of the composition of water.

* In a note to his Memoir of the 26th of November, 1783 (p. 331.), we read the following remark by Watt: "Anterior to Dr. Priestley's experiments, Kirwan had proved, by some ingenious deductions borrowed from other facts, that inflammable air is in all probability the true phlogiston in an aërial form. Kirwan's arguments appear to me perfectly convincing; but it seems more suitable to establish this point of the question by direct experiments."

† At the foot of p. 333. of the *Transactions* (for 1784), in a part of his April letter, 1783, printed in italics, Watt said: "Are we not then authorised to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated air, or pure air, is composed of water deprived of its phlogiston and united to elementary heat and light; that heat and light are contained in it in a latent state, since they do not affect either the thermometer or the eye? If light is only a modification of heat, or a peculiarity in its existence, or a constituent part of inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston, and united to some elementary heat."

Is not this passage as clear, precise, and intelligible as Lavoisier's conclusions? —(*Note by Mr. Watt, jun.*)

The obscurity complained of by Lord Brougham in the theoretical conceptions of Watt and of Cavendish appears to me unfounded. In 1784 they knew how to prepare two permanent and very dissimilar gases. Those two gases were by some distinguished as pure air and inflammable air; by others as dephlogisticated air and phlogiston; by others, finally, as oxygen and hydrogen. By the combination of dephlogisticated air and phlogiston, they generated water weighing as much as the two gases. Thenceforward water was no longer a simple body; it was composed of dephlogisticated air and phlogiston. The chemist who deduced this conclusion might have false ideas on the internal nature of phlogiston without its casting any uncertainty on the merit of his first discovery. Has it been even now *mathematically demonstrated* that hydrogen (or phlogiston) is an elementary body; that it is not, as Watt and Cavendish for a time supposed, the combination of a radical with a little water? — (*Note by M. Arago.*)

2. Watt established this theory during the year 1783, in more distinct terms than Cavendish did in his Memoir read to the Royal Society in January, 1784. By noticing also the disengagement of latent heat in the operation, Watt added very much to the clearness of his conception.

3. There is no proof, there is not even any assertion whence it would result, that the *theory* of Cavendish (Blagden calls it *conclusion*) was communicated to Priestley previously to Watt's delivering his ideas in the letter of the 26th of April, 1783; and still more, nothing leads one to suppose, especially after reading Watt's letter, that he had ever heard anything relative to the composition of water either from Priestley or from any other person.

4. Watt's theory was known by the Fellows of the Royal Society *several* months before the conclusions of Cavendish had been committed to paper: *eight* months before the Memoir of that chemist was presented to that same Society. We can go farther, and deduce from facts and dates now before us, that Watt was the first to speak of the composition of water; that if any one was anterior to him, there is no proof of it.

5. Finally, a repugnance to abandon the doctrine of phlogiston, a sort of timidity in separating from an opinion so long established, so deeply rooted, prevented Watt and Cavendish from rendering complete justice to their own theory; whilst Lavoisier, who had broken through those trammels, was the first to present the new doctrine in all its perfection.*

It might be very possible that without knowing anything of their respective labours, Watt, Cavendish, and Lavoisier had nearly at the same time made the great step of concluding from experiment, that water is the product of a combination of the two gases so often quoted. Such is, in short, with more or less distinctness, the conclusion that the three learned men presented.

It now remains to consider Blagden's declaration, from which Lavoisier might have learned the theory of Cavendish, even before he had made his principal experiment. Blagden inserted this declaration in Cavendish's own Memoir †; it was published in the *Philosophical Transactions*, and it does not appear that Lavoisier ever contradicted it, however irreconcileable it was with his own recital.

Notwithstanding all Blagden's susceptible jealousy in favour of the

* No one ought to have expected from Watt, writing and publishing for the first time, exposed to the cares of an immense manufactory and of commercial affairs equally extensive, that he could vie with the eloquent and practised pen of Lavoisier; but the substance of his theory (see p. 333. of his Memoir) seems, at least to me, who in truth may not perhaps be an impartial judge, as luminous and as remarkable in expression as the conclusions of the illustrious French chemist. — (*Note by Mr. Watt, jun.*)

† A letter to Professor Crell, in which Blagden gave a detailed account of the discovery, appeared in the *Annalen* of 1786. It is remarkable that in this letter Blagden says that he communicated to Lavoisier the opinions of Cavendish *and of Watt*, and that this latter name figures here for the first time in the confidential verbal recitals of the Secretary of the Royal Society.— (*Note by Mr. Watt, jun.*)

priority of Cavendish, there has not been on his part a single allusion from which one might deduce that before publishing his own, Watt had heard of his competitor's theory.

We will not be so positive relative to the question of Cavendish having had some knowledge of Watt's labours, before arranging the conclusions in his own Memoir. To maintain that Cavendish was not ignorant of Watt's conclusions, we might remark how very improbable it was that neither Blagden nor any one else to whom those conclusions were known, had ever mentioned them to him. It might also be said that Blagden, even in those portions of the Memoir that were written in his own hand, and intended to claim the priority for Cavendish against Lavoisier, nowhere affirms that the theory of Cavendish was conceived before the month of April, 1783, although in another addition to his friend's original Memoir there is a quotation relative to Watt's theory.

Since the question as to the epoch when Cavendish drew conclusions from his experiments, is enveloped in great obscurity, it may be of some utility to inquire what were this chemist's habits when he communicated his discoveries to the Royal Society.

A Committee of that Society, to which Gilpin belonged, made a series of experiments on the formation of nitric acid. This Committee, placed under the direction of Cavendish, sought to convince those who doubted of the composition of the acid in question, incidentally indicated in the Memoir of January, 1784, and afterwards more at length in a Memoir of June, 1785. The experiments were made between the 6th of December, 1787, and the 19th of March, 1788. The date of the reading of Cavendish's Memoir was the 17th April, 1788. The reading and the printing then occurred within less than a month of the completion of the experiments.

Kirwan presented his objections to Cavendish's Memoir relative to the composition of water, on the 5th of February, 1784. Cavendish's answer was read on the 4th of the following March.

The experiments on the density of the earth occupied the interval between the 5th of August, 1797, and the 27th of May, 1798. The date of the reading of that Memoir was the 27th of June, 1798.

In the Memoir on the eudiometer, the experiments quoted were made in the latter half of 1781, but the Memoir was not read till January, 1783. Here the interval was greater than in the preceding communications. From the nature of the subject, however, it is probable that the author undertook fresh experiments in 1782.

Everything renders it probable that Watt conceived his theory in the course of a few months or even of a few weeks prior to April 1783. It is certain that this theory was considered by him as his property, for he did not allude to any anterior or analogous communication; nor does he say that he had heard of Cavendish having come to similar conclusions.

We cannot believe that Blagden would have heard no mention of Cavendish's theory prior to the date of Watt's letter, if that theory had actually preceded the letter, and that he would not have been eager to point out this circumstance in the additions that he made to his friend's Memoir.

It is well finally to remark that Watt depended entirely on Blagden's taking care to correct the proofs, and attending to everything else that could relate to the printing of his Memoir. This is proved by a letter, still existing, addressed to Blagden. Watt saw his Memoir only after it had been printed.

The notes by Mr. Watt, jun., made part of a manuscript which was sent me by Lord Brougham; and it is at the express request of my illustrious co-academician that I have had them printed as a useful commentary on his work.

NOTE BY W. FAIRBAIRN, F.R.S. F.G.S.

In writing his historical eulogy of James Watt, the distinguished French philosopher has allowed his partiality for his own countrymen to overstep the boundaries within which an impartial writer should be restrained. To associate Dr. Papin as a coadjutor of Watt in the discovery and invention of the steam-engine, is to give to the former a degree of prominence to which he is certainly not entitled; and it is much to be regretted, that men in so high a position as Arago, with minds so imbued with the love of truth, and the desire to award to individual merit the praise justly due to labours in the field of discovery, should be so biassed by love of country as to endeavour to curtail the merits, and to divide the honour which exclusively belongs to one who has done more for practical science, and for the great family of mankind, than any other person since the days of Newton.

Papin was contemporary with Newton, and laboured in the same field as Savery, in experiments on the effects of steam as a motive power; but we have yet to learn that that power was ever applied by him to the organic parts of an engine, calculated to overcome the resistance of a load, such as the propulsion of machinery or the raising water from mines. The discovery of an element of power is a totally different thing to its application through the organic parts of a machine. The first is the result of experimental research in the laboratory; the second is the result of toilsome labour in the workshop, in the actual production of a machine: merit for the former belongs to Dr. Papin, for the latter, exclusively to Newcomen and James Watt.

Savery constructed an engine for raising water upon the principle of condensation. It consisted of two vessels,—a boiler and a condenser, if we may so term them, the latter being connected by pipes with the water in the mine and the reservoir to which it was to be raised. Under the boiler a fire was lighted, and the steam was allowed to fill the condenser; the connection with the boiler was then cut off, and a

jet of cold water thrown into the condenser, which at once created a vacuum; the pressure of the atmosphere now forced the water from a depth not exceeding 30 feet in the mine, into the condenser, where it was retained by a valve. Steam from the boiler then forced the water from the condenser upwards through the pipe to the reservoir above, and as soon as it was again filled with steam, the process was repeated. This slow and tedious operation was regulated by hand, but that could only be done under the limits stated above, and with an enormous consumption of fuel. This was the apparatus adopted by Savery, but we have no satisfactory information that Dr. Papin ever constructed an engine worked by steam; his attempts were made on models which were never usefully applied; and Dr. Hooke, in his correspondence with Newcomen and the Royal Society, pointed out the absurdity and fallacy of the air-pipes and pistons, which he proposed as a means of raising water from mines. The only real inventor, antecedent to Watt, was Newcomen, who introduced the open top cylinder, and the reciprocating motion of the piston and beam. The apparatus antecedent to this scarcely deserved the name of *engine*, still less should it be considered the parent of the modern steam-engine. Newcomen's engine first acquired the character of an automaton (however rudely formed) from the ingenious application of the boy Potter, while its subsequent developments, far surpassing in number and importance all that had preceded, are exclusively due to James Watt. Newcomen invented the engine as it was when Watt repaired the far-famed model belonging to the Glasgow University—a mere pumping machine; Watt made it a source of motive power capable of application in every situation and for every kind of work; and it was in his hands that it received the name and properties it now possesses, as the most extraordinary invention of all time.

Arago arrogates to Papin the merit as if his discoveries had led to the mechanical arrangements of the steam-engine, or to the invention of condensation in a separate vessel. Now it is evident that Watt was not in any way indebted to him, even for a hint in the attainment of these results. Papin was not even capable of devising the mechanical arrangements of an engine, as it issued from the hands of Watt; and even Newcomen's was so rude an attempt, that the present steam-engine may be safely considered as the exclusive invention of James Watt.

It is highly interesting and exceedingly curious to trace the progressive developments of this machine, as it acquired, by slow but certain stages, its present proportions and power. The constant study, unwearied application, and experimental research which distinguished every step made in advance, will, to the end of time, uphold the name, and exhibit the untiring energy, of the man who produced so important — so various results.

It is unbecoming in a great man and a great nation to attempt to drag forward competitors where no competition exists,—where, in fact, the inventor stands alone as the benefactor of the human race. If Watt had done no more than the introduction of his condenser, whereby he gained one of the greatest steps towards making the engine what it is, quadrupled its power, and gave to it the docility and powers of adapta-

tion of almost animal existence, — if he had done no more than this, he was entitled to a world's gratitude, and to all the honours of an original inventor. But it was not this that marked the fertility of his mind, but the perfection of his engine by an organisation which has made it so powerful and yet so perfect, which has given it a smoothness of action and almost vital adaptibility to every kind of work, and which will ever excite the admiration of every mind conversant with the beauty of mechanical design.

So valuable an invention did not escape constant piracy. Engines began to be erected on the principles of separate condensation, which not only infringed the patent rights, but from their miserable construction brought discredit on engines as a class; so that Boulton and Watt were compelled at last, however reluctantly, to commence a long series of legal proceedings, which at length fully established the validity of their patent.

To show the progress that was made in the construction of engines, and the immense importance of their manufacture, we quote from a recent work on the *Mechanical Inventions of James Watt*, by James Muirhead, Esq., the number of engines constructed at Soho.

" *Memorandum.*

" Soho Foundry, 16th March, 1854.

" The number and power of the engines made by Messrs. Boulton, Watt, and Co., to the date January, 1824, were thus reckoned by the late Mr. Boulton and Mr. Creighton (one of his assistants at Soho): —

Engines.	Nominal Horse-power.	Power of living Horses required to do the same work.
283 for pumping and blowing	11,247 × 4	44,988
805 rotative	12,618 × 3	37,854
76 boat engines	2,080 × 3	6,240
1164	25,945	89,082

" And between January 1824 and January 1854, the numbers are the following: —

34 for pumping and blowing	2403 × 4	9,612
164 rotative	7517 × 3	22,551
243 boat engines	15,358 × 3	46,074
441	25,278	78,237

" Giving the following total numbers: —

1164	25,945	89,082
441	25,278	78,237
1,605	51,223	167,319

" The first engine seems to have been made for Bedworth, in 1776."

It will be noticed that for pumping engines the nominal horse-power is multiplied by four to give the real horse-power required to do the

same work in the same time; and this is on the supposition that a horse can work only six hours a day, whilst the engine can work twenty-four. But in rotative engines an allowance has been made in the above table for loss of power in the action of the crank, &c. as compared with the direct action in the other case, and the nominal horse-power is multiplied by three only.

Perhaps it would be more accurate to suppose that a horse can work for eight hours out of the twenty-four; but at the same time to multiply the nominal horse-power by two, because each indicated horse-power of the engine = 33,000 lbs. raised one foot high per minute, is at least twice as much as its nominal power, or twice as much as an ordinary horse could work up to. We shall then find that it would require no less than 250,974 living horses to perform the work of the engines constructed by Messrs. Boulton and Watt up to January 1854.

One of Watt's fellow labourers should hardly be passed over in any statement connected with the steam-engine; we allude to the late Mr. William Murdock, whose vast practical knowledge was employed in carrying out the designs of Watt for upwards of half a century. Mr. Murdock directed the application of the new steam-engines to drain the water of the Cornish mines. In order to adapt that moving power to exhausting pumps, and to establish the system in mines of extreme depth innundated by appalling quantities of water, great skill in practical mechanics was requisite. Mr. Murdock showed that he had sufficient resources of genius and wisdom of experience to triumph over every difficulty. He was the introducer of the system of lighting by coal gas, and for his paper on that subject sent to the Royal Society he received the Rumford gold medal. He was also the patentee of some new methods of constructing steam-engines, &c., and his suggestions often enriched the Soho machinery. We have therefore great pleasure in bearing testimony to the merits of one of our first practical mechanics, the able assistant and coadjutor of James Watt.

W. F.

THE END.